

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

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| TITLE (PROVISIONAL) | A cross-sectional study of cognitive stress appraisal and related factors among workers in metropolitan areas of Japan. |
| AUTHORS | Tohmiya, Natsuka; Tadaka, Etsuko; Arimoto, Azusa |

VERSION 1 – REVIEW

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| REVIEWER | Rui Gomes Universidade Minho Escola de Psicologia Portugal |
| REVIEW RETURNED | 04-Oct-2017 |

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| GENERAL COMMENTS | <p>Line 153. Why does the psychological measure of depression is identified as a “demographic characteristic”? It should be a “psychological characteristic. And is there any data about the psychometric properties of this instrument?</p> <p>Line 158. What does it mean a “high average score”? it would be better to provide specific “cut-off” values, or if they do not exist then it should only be said that higher scores correspond to higher tendency to depression.</p> <p>Line 166. Some of the independent variables appears for the first time in this section (as for example “disease currently under treatment”, “body mass index (BMI)”, and they were analyzed in the introduction section, meaning that we cannot understand clearly the relation with the topic of this study.</p> <p>Line 174. Again, any indication of psychometric properties was provided for the BJSQ and for the JCQ, lifestyle, PHCS, eHEALS, and Organizational Climate Scale, MPSS.</p> <p>Authors seem to use an protocol evaluation with a lot of instruments, and this may explain the relatively low return rate of this study. Did they control this aspect in the study?, if not this should be addressed as a limitation of the study.</p> <p>Line 257. This paragraph should be include in the Data collection (line 230).</p> <p>Line 282. It would be important to provide some indicators of adjustment for the regression, as normality, outliers, collinearity...</p> |
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| REVIEWER | Dr. Richa Gupta Mahatma Gandhi Medical College and Research Institute, Sri Balaji Vidyapeeth, Pondicherry India |
| REVIEW RETURNED | 20-Nov-2017 |

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| GENERAL COMMENTS | The paper aims to identify demographic, individual and environmental factors contributing to stress so that they can be minimized by interventions. However, I have few concerns with this |
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| | <p>version of the paper</p> <ol style="list-style-type: none"> 1. Measurement of depression: It is not clear whether depression scale used is for group as a whole as one line states " A group with high average score may be interpreted to be at-risk of depression" . It talks about a 'group' and it does not state how much 'high average score' constitutes at risk. 2. Why is depression put under demographic characteristics? 3. The use of term positive or negative stress appraisal is misleading as it appears there is a cut-off over which appraisal is considered negative and below which it is considered positive. On the contrary, PSS scale is a continuous scoring scale. 4. Were independent variables checked for the presence of multicollinearity before applying multiple regression analysis? 5. Stratified random sampling was used for recruiting the study participants. But in results it's not mentioned whether the participants who responded belonged to all strata. This will affect the generalizability of the study. 6. Table 2 is not provided. Instead table 3 is mentioned as table 2 7. Explain what was done to increase response rate. If it was not feasible, is it a factor which can affect the interpretation and generalizability of the study. For e.g. if response rate was more from one strata of population as compared to other, it may be due to inherent variability in stress perception. People experiencing more stress may be inclined to respond as compared to people experiencing less stress. 8. How did the authors reach to the conclusion (in conclusion section) that cognitive stress appraisal was associated with depression. In the present study, they have assumed it to be a covariate and used it as controlled variable. <p>General comments:</p> <ol style="list-style-type: none"> 1. Kindly mention that only health related individual factors were taken into account |
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| REVIEWER | Andrew J Wawrzyniak University of Miami Miller School of Medicine Psychiatry & Behavioral Sciences, USA |
| REVIEW RETURNED | 27-Nov-2017 |

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| GENERAL COMMENTS | <p>The manuscript "An epidemiological study of cognitive stress appraisal and related factors among workers: cross-sectional study" reports on the relationship between individual and environmental factors associated with cognitive stress appraisal in Japanese workers in 337 respondents.</p> <p>--Overall comments -Overall, well-written and clearly presented. There are a few grammatical errors, but this can be easily fixed in the copy editing process. -The manuscript occasionally mentioned public health nurses and</p> |
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health practitioners. Although this is an important point and these healthcare professionals would be ideal for future intervention work, it sometimes seems like their role is thrown in. This can either be omitted or should be built up and explained more clearly. For example, do you envision your results to directly impact any initiatives designed for stress reduction? If so, then that explains why you mention these healthcare practitioners.

--Abstract

-No major edits needed.

--Introduction

-See the statement above regarding healthcare practitioners - this concept seems thrown in at the end of the 2nd paragraph in the Introduction.

--Methods

-Move the section on Ethical Approval to the beginning of the Methods section.

-Specify how demographics were asked - were they standard questions used in other questionnaires? Questions & categories generally used by the government/census?

--Results

-The text refers to Table 1, 2, and 3, but there is no Table 3. Table 2 should be numbered Table 3 and Table 2 with the correlations should be added to the manuscript.

-Please mention that BMI is calculated from self-reported weight and height.

-Detail abbreviations for questionnaires at the end of Table 1 - say what PHCS means, for example.

-Certain categorical variables included in the regression should be clarified - were these dichotomized? For example, marital status was reported to have an $r = -0.207$, but marital status has 3 categories - Unmarried, Married, and Divorced/Widowed. Which one is which and how were they coded? Was Unmarried, for example, equal to 1? Furthermore, how is Unmarried "lower" than Divorced/Widowed if $\text{Divorced/Widowed} = 3$?

-It may be clearer to indicate you performed a 3 step regression, with step 1 have the control variables, step 2 having the demographics, and step 3 having the remaining predictors.

-This paper would benefit from a power analysis.

-The adjusted R^2 is reported inconsistently - it is reported in 3 different places as 0.412, 0.411, and 0.409. Check this.

-This manuscript needs a missing data analysis to indicate if the questionnaires not sent back are missing at random/missing completely at random/missing due to an unmeasured variable.

--Discussion/Conclusions

-Further develop the organizational climates in the 5th paragraph of the Discussion - this is a very important point.

-You can leave out the sections before the References regarding Patient Consent, Ethics Approval, Provenance and Peer Review, and Data Sharing Statement unless required by the journal, although

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| | <p>it was not mentioned in the checklist.</p> <p>-References -There may be references missing or a mistake in the numbering. The reference list ends at #69, but in the text, there is reference #74. Please check this.</p> |
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VERSION 1 – AUTHOR RESPONSE

[Reply to Editorial Requests]

1. Please revise your title to indicate the research question, study design, and setting. This is the preferred format of the journal.

→I’m sorry to my mistake. In the first cover letter for you, our study title was not written “:cross-sectional study.” However, file of main document was written the title.

2. Please work to improve the quality of the English throughout your manuscript. We recommend asking a native English speaking colleague to assist you or seek the help of a professional copy-editing service.

→The paper has been edited and rewritten by an experienced scientific editor, who has improved the grammar and stylistic expression of the paper.

[Reviewers' Comments to Author]

Reply to Reviewer 1(Dr. Rui Gomes)

1. Line 153. Why does the psychological measure of depression is identified as a “demographic characteristic”? It should be a “psychological characteristic. And is there any data about the psychometric properties of this instrument?

→Cognitive stress appraisal is affected by participants mental condition at that time. Depression is basic mental condition of participants. Moreover it is not independent variable because of treating as a control variable. For the reason, we put it under demographic characteristics. Moreover, the psychometric properties of CES-D were confirmed reliability and validity. CES-D had high internal consistency, acceptable test-retest stability, and excellent concurrent validity by clinical and self-report criteria and substantial evidence of construct validity. When CES-D designed, internal consistency was high in the general population (0.77-0.87) and even higher in the patient sample (0.85-0.92). And the test-retest correlations were in the moderate range (between 0.45 and 0.70). In addition, the correlations of the CES-D with the Hamilton Clinician’s Rating scale and with the Raskin Rating scale were moderate (0.44-0.54) at admission. (L.179-183)

2. Line 158. What does it mean a “high average score”? it would be better to provide specific “cut-off” values, or if they do not exist then it should only be said that higher scores correspond to higher tendency to depression.

→In accordance with your advice, I changed the expression. (L.178-183)

3. Line 166. Some of the independent variables appear for the first time in this section (as for example “disease currently under treatment”, “body mass index (BMI)”, and they were analyzed in the introduction section, meaning that we cannot understand clearly the relation with the topic of this study.

→In this study, we selected independent variables by reference to previous studies. A part of PSS at introduction, we mentioned “previous studies clarified various individual factors related to the PSS, but varied for different participants.” Conceptual framework of this study was to examine cognitive stress appraisal and identify individual and environmental factors. According to Lazarus’s theory, individual and environment mutually affect in cognitive stress appraisal process. So, we thought both individual

and environmental factors were important. When we selected independent variables, we referred previous studies. In previous studies, demographic characteristics were incorreced. We chose not necessarily confirmed relationship between cognitive stress appraisal and independent variables and a lot used in previous studies. (L.192-195)

4. Line 174. Again, any indication of psychometric properties was provided for the BJSQ and for the JCQ, lifestyle, PHCS, eHEALS, and Organizational Climate Scale, MPSS.

Authors seem to use an protocol evaluation with a lot of instruments, and this may explain the relatively low return rate of this study. Did they control this aspect in the study?, if not this should be addressed as a limitation of the study.

→Thank you. I addressed a lot of instruments may affect return rate in limitation section (L.404-405).

5. Line 257. This paragraph should be include in the Data collection (line 230).

→In accordance with your advice, I include in the part.

6. Line 282. It would be important to provide some indicators of adjustment for the regression, as normality, outliers, collinearity...

→When we analyzed, independent variables was considered multicollinearity. I added this sentence in manuscript (L.274-275).

Reply to Reviewer 2 (Dr. Richa Gupta)

1. Measurement of depression: It is not clear whether depression scale used is for group as a whole as one line states "A group with high average score may be interpreted to be at-risk of depression." It talks about a 'group' and it does not state how much 'high average score' constitutes at risk.

→ In accordance with your advice, I changed the expression. (L.173)

2. Why is depression put under demographic characteristics?

→Cognitive stress appraisal is affected by participants mental condition at that time. Depression is basic mental condition of participants. Moreover it is not independent variable because of treating as a control variable. For the reason, it put under demographic characteristics. I added sentences of reason for depression putting under demographic characteristics (L.174-176).

3. The use of term positive or negative stress appraisal is misleading as it appears there is a cut-off over which appraisal is considered negative and below which it is considered positive. On the contrary, PSS scale is a continuous scoring scale.

→We focused on more negative cognitive stress appraisal. Cohen said that PSS is a measure of the degree to which situations in one's life are appraised as stressful. In addition, items of PSS were designed to tap how unpredictable, uncontrollable, and overloaded respondents find their lives and these issues have been repeatedly found to be central components of the experience of stress. (Perceptions of stress and negative affect are necessary for stressful life events to influence disease risk.) Also, Cohen said PSS can be used to determine whether "appraised" stress is an etiological (or risk) factor in behavioral disorders or disease. Thus, we interpreted PSS can continuously measure negative cognitive stress appraisal. (L.102-110)

4. Were independent variables checked for the presence of multicollinearity before applying multiple regression analysis?

→Thank you. Our choice of independent variables was considered multicollinearity. I added this sentence in manuscript (L.274-275).

5. Stratified random sampling was used for recruiting the study participants. But in results it's not mentioned whether the participants who responded belonged to all strata. This will affect the generalizability of the study.

→Thank you for your advice. This study used how to maintain the anonymity of all personal participant information including company's name, type of industry, and so on. So we cannot mention that participants were belonged to the strata. However, we selected companies stratified number of employees based on Industrial Safety and Health Act. Moreover, we clarified that there is no biased type of industry. (L.138-140)

6. Tumble 2 is not provided. Instead table 3 is mentioned as table 2

→This error has been corrected in accordance with the reviewer's comment.

7. Explain what was done to increase response rate. If it was not feasible, is it a factor which can affect the interpretation and generalizability of the study. For e.g. if response rate was more from one strata of population as compared to other, it may be due to inherent variability in stress perception. People experiencing more stress may be inclined to respond as compared to people experiencing less stress.

→Thank you for your advice. We added an explanation about how to increase response rate at part of limitation. (L.404-405)

8. How did the authors reach to the conclusion (in conclusion section) that cognitive stress appraisal was associated with depression. In the present study, they have assumed it to be a covariate and used it as controlled variable.

→We think it means making a negative cognitive stress appraisal can affect poorer mental health. Our study aimed to gain practice suggestions of primary prevention. The relationship between cognitive stress appraisal and depression is shown significant correlations in common with previous studies. Depression was considered poorer mental health, and can affect cognitive stress appraisal. For that reason, we treated depression as control variables.

Moreover, PSS measures the degree to which situations are cognitively appraised as stressful. It was said that when intended as a predictor of psychological distress, PSS was most appropriately used in prospective studies that control for initial distress level by Cohen. Perception of stress is influenced mental health including depression. (L.102-110)

General comments:

9. Kindly mention that only health related individual factors were taken into account.

→Thank you. We added to discussion about organizational climate. (L.377-384)

Reply to reviewer 3 (Dr. Andrew J Wawrzyniak)

1. Overall comments

-Overall, well-written and clearly presented. There are a few grammatical errors, but this can be easily fixed in the copy editing process.

→Thank you very much. We fixed in accordance with the reviewer's comment.

2. -The manuscript occasionally mentioned public health nurses and health practitioners. Although this is an important point and these healthcare professionals would be ideal for future intervention work, it sometimes seems like their role is thrown in. This can either be omitted or should be built up and explained more clearly. For example, do you envision your results to directly impact any initiatives designed for stress reduction? If so, then that explains why you mention these healthcare practitioners.

→Thank you for your advice. We omitted the explanation about public health nurses and health practitioners.

3. Abstract

-No major edits needed.
→Thank you very much.

4. Introduction

-See the statement above regarding healthcare practitioners - this concept seems thrown in at the end of the 2nd paragraph in the Introduction.
→Thank you for your advice. We did not change the Introduction because we deleted the statement above regarding healthcare practitioners.

--Methods

5. -Move the section on Ethical Approval to the beginning of the Methods section.
→Thank you. However, we wrote Ethics into Footnotes. (L.446-450)

6. -Specify how demographics were asked - were they standard questions used in other questionnaires? Questions & categories generally used by the government/census?
→We asked standard questions generally used in previous study for workers. Furthermore, we deliberated about items of national survey for workers –e.g. The Stress Check Test, Basic Survey on Industrial Safety and Health. (L.166-167)

--Results

7. -The text refers to Table 1, 2, and 3, but there is no Table 3. Table 2 should be numbered Table 3 and Table 2 with the correlations should be added to the manuscript.
→This error has been corrected in accordance with the reviewer's comment.

8. -Please mention that BMI is calculated from self-reported weight and height.
→Thank you. We added this sentence. (L.197)

9. -Detail abbreviations for questionnaires at the end of Table 1 - say what PHCS means, for example.
→We mentioned detail abbreviations for questionnaires at independent variables in methods before Table 1. So we used abbreviation at Table 1. However, if abbreviations' mean (e.g. Depression) were difficult to understand, we told about detail that not same item and scale name at Table1.

10. -Certain categorical variables included in the regression should be clarified - were these dichotomized? For example, marital status was reported to have an $r = -0.207$, but marital status has 3 categories - Unmarried, Married, and Divorced/Widowed. Which one is which and how were they coded? Was Unmarried, for example, equal to 1? Furthermore, how is Unmarried "lower" than Divorced/Widowed if Divorced/Widowed = 3?
→Thank you. It is as you have pointed out. We added an explanation about categorical variables. (L.159-166)

11. -It may be clearer to indicate you performed a 3 step regression, with step 1 have the control variables, step 2 having the demographics, and step 3 having the remaining predictors.
→This error has been corrected in accordance with the reviewer's comment.(L.277-278)

12. -This paper would benefit from a power analysis.
→We conducted a power analysis as follows, and we added this explanation. Sample size was calculated using sample-size calculating software G*Power version 3.0.10 (Faul, Erdfelder, Lang, & Buchner, 2007). With power of 80 %, 0.05 statistical level of significance, effect size of 0.15(Cohen, 1992) and number of predictors of 13, sample size for multiple regression model was calculated to be 131. (L.283-288)

Cohen J. A Power Primer. Psychological Bulletin. 1992; 112(1): 155-159.

Faul F, Erdfelder E, Lang AG, Buchner A. G*Power 3: a flexible statistical power analysis program for the social, behavioral, and biomedical sciences. Behav Res Methods. 2007;3:175-91.

13. -The adjusted R2 is reported inconsistently - it is reported in 3 different places as 0.412, 0.411, and 0.409. Check this.

→Thank you for your comment. I rewrote the adjusted R2 .

14. This manuscript needs a missing data analysis to indicate if the questionnaires not sent back are missing at random/missing completely at random/missing due to an unmeasured variable.

→We converted a missing data into a median value of this study sample. (L.287)

--Discussion/Conclusions

15. -Further develop the organizational climates in the 5th paragraph of the Discussion - this is a very important point.

→We added to discussion of organizational climates. (L 377-384)

16. -You can leave out the sections before the References regarding Patient Consent, Ethics Approval, Provenance and Peer Review, and Data Sharing Statement unless required by the journal, although it was not mentioned in the checklist.

→Thank you for your supportive comments. We deleted those heads.

-References

17. -There may be references missing or a mistake in the numbering. The reference list ends at #69, but in the text, there is reference #74. Please check this.

→This error has been corrected in accordance with the reviewer's comment.

VERSION 2 – REVIEW

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| REVIEWER | Richa Gupta Mahatma Gandhi Medical College and Research Institute, India |
| REVIEW RETURNED | 12-Jan-2018 |

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| GENERAL COMMENTS | <p>Authors have addressed most of the queries. However, still some concerns remain</p> <ol style="list-style-type: none">1. Authors have changed the statement to Line 138-140: We selected companies stratified number of employees based on... Moreover we clarified there is no biased type of industry” probably in response to query on Stratified random sampling. It is not clear what do they mean by “stratified number of employees” and “no biased type of industry”2. They have removed the term stratified random sampling. So what kind of sampling was this. If it was stratified random sampling, why was response rate not categorized in strata and analyzed accordingly.3. In line 287, authors state missing data was converted into median value. How much percentage of the response was missing date?4. The added statements are incoherent with rest of the paper e.g. sample size calculation was mentioned after rest of statistical analysis followed by missing values. |
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| | Missing values should come at line 269 before mean and SD calculation. Similarly sample size calculation in sampling. 5. English of added statements needs to be corrected. It will make the paper more legible |
| REVIEWER | Rui Gomes university of minho. portugal |
| REVIEW RETURNED | 17-Jan-2018 |
| GENERAL COMMENTS | authors made the changes required on first revision. |

VERSION 2 – AUTHOR RESPONSE

[Reply to Editorial Requests]

1. The title still does not contain the study setting. Please amend to something like: “A cross-sectional study of cognitive stress appraisal and related factors among workers in two metropolitan areas of Japan.”

→At the reviewer’s suggestion, we have reset the title as “A cross-sectional study of cognitive stress appraisal and related factors among workers in metropolitan areas of Japan.”

2. The quality of English still needs improving in places. Some examples are included below (however, please note this is not an exhaustive list):

page 2 - “This study is cross-sectional design, it could not identify causal relationships between cognitive stress appraisal and related factors.”

Page 5 – “We asked standard questions generally used in previous study for workers and deliberated about items of national survey for workers” Do you mean “previous studies”? This sentence needs to be a lot clearer in general.

Page 9 – “We converted a missing data into a median value of this study sample” Please improve the quality of English in this sentence.

Please thoroughly copy-edit the manuscript again.

→The manuscript has been edited by a professional native English editing (edanz; https://www.edanzediting.co.jp/?gclid=EAlaIqobChMIINuAuce72QIVDAoqCh3ChAMzEAAYASAAEgKYa_D_BwE), so we hope it now matches the journal standard.

3. Please also justify your decision to convert missing data into median values (see response to reviewer 3, comment 14)

→We are sorry, we mistook our last manuscript about the handling of missing data. The correction is as follows; Of the 337 effective response, data was missing for; BMI (n=2, 0.59%), self-rated health (n=14, 4.15%), household membership (n=3, 0.89%), employment status (n=2, 0.59%), and CES-D (n=10, 3.20%), therefore, these cases were excluded from the multiple regression model(L.277-280).

[Reviewers' Comments to Author]

Reply to Dr. Rui Gomes

1. authors made the changes required on first revision.

→Thank you very much.

Reply to Dr. Richa Gupta

1. Authors have changed the statement to Line 138-140: We selected companies stratified number of employees based on... Moreover we clarified there is no biased type of industry” probably in response to query on Stratified random sampling. It is not clear what do they mean by “stratified number of employees” and “no biased type of industry”

→Please excuse our error in the last manuscript. The correction is “stratified random sampling.” First, we stratified companies’ size and type of industry, and then selected companies randomly within that stratification. (L.145-149)

2. They have removed the term stratified random sampling. So what kind of sampling was this. If it was stratified random sampling, why was response rate not categorized in strata and analyzed accordingly.

→The sampling was “stratified random sampling.” First, we stratified companies by size and type of industry. Then, we selected companies randomly within that stratification. The response rate was not categorized in strata and analyzed accordingly because the questionnaire did not collect details about companies’ name, the number of employee and type of industry to safeguard participant anonymity. (L.145-149)

3. In line 287, authors state missing data was converted into median value. How much percentage of the response was missing data?

→We are sorry, we mistook our last manuscript about the handling of missing data. The correction is as follows; Of the 337 effective response, data was missing for; BMI (n=2, 0.59%), self-rated health (n=14, 4.15%), household membership (n=3, 0.89%), employment status (n=2, 0.59%), and CES-D (n=10, 3.20%), therefore, these cases were excluded from the multiple regression model(L.277-280).

4. The added statements are incoherent with rest of the paper e.g. sample size calculation was mentioned after rest of statistical analysis followed by missing values.

Missing values should come at line 269 before mean and SD calculation. Similarly sample size calculation in sampling.

→We rearranged the part of Data collection. (L.151-161)

5. English of added statements needs to be corrected. It will make the paper more legible

→The manuscript has been edited by a professional native English editing (edanz; https://nec.edanzediting.com/?utm_expnid=.RYVLVLjcTHqL8av48ICWUA.1&utm_referrer=https%3A%2F%2Fwww.google.co.jp%2F), so we hope it now matches the journal standard.

VERSION 3 – REVIEW

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| REVIEWER | Richa Gupta Mahatma Gandhi Medical College and Research Institute, India |
| REVIEW RETURNED | 26-Mar-2018 |

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| GENERAL COMMENTS | Congratulations with this version of the paper |
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