

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) | Associations of disaster-related and psychosocial factors with changes in smoking status after a disaster: A cross-sectional survey after the Great East Japan Earthquake |
| AUTHORS | Nakano, Hironori; Ohira, Tetsuya; Maeda, Masaharu; Yabe, Hirooki; Ohtsuru, Akira; Suzuki, Yuriko; Harigane, Mayumi; Horikoshi, Naoko; Nagai, Masato; Zhang, Wen; Takahashi, Hideto; Yasumura, Seiji; Iso, Hiroyasu; Kamiya, Kenji |

VERSION 1 – REVIEW

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| REVIEWER | Taishi TSUJI Chiba University, Japan |
| REVIEW RETURNED | 10-Oct-2017 |

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| GENERAL COMMENTS | <p>This study explores the changes in smoking status and the related factors among the residents of the evacuation area in Fukushima after the Great East Japan Earthquake. Overall, the conclusions and findings of this study are supported by the qualitative and quantitative data, but the manuscript still needs a few revision to be acceptable for the BMJ Open as the Research Article.</p> <p>Abstract Conclusion: Although the Authors described “The proportion of smokers decreased slightly among residents in the evacuation area”, there is no description corresponding to it in the Results section of the Abstract.</p> <p>Conclusion: “the quitting of smoking after...” → “the starting of smoking after...”</p> <p>Methods P8 L17: “11 March 11 2011” → “11 March 2011”</p> <p>P10 L13: As for the socioeconomic and disaster-related variables (i.e., living arrangement, experienced living in evacuation shelters, whether house was damaged, and whether experienced tsunami), these look like depending on each other. Could there have been any multicollinearity effects?</p> <p>Discussion P15 L14: The related factors on changes in smoking status were not well discussed compared to discussions on the decrease in proportion of current smokers. Furthermore, “Therefore, to prevent people from starting smoking after a disaster, effective management of traumatic symptoms and non-specific psychological distress may be required”, this needs some better formulation.</p> |
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| | <p>P15 L17-P16 L10: Even though the cigarette/tobacco sales significantly decreased (ref. no. 25), why the proportion of smokers in Japan slightly increased from 19.5% in 2010 to 20.7% in 2012 (rather than no change) between before and after the disaster (ref. no. 24)? Is your speculation: “these situations probably attributed to the smokers quitting smoking” true? Could you comment on this?</p> <p>P17 L3: As the Authors stated in the limitation paragraph, the response rate was relatively low. Is there possibility that the proportion of smokers after the disaster was underestimated because non-respondents tend to be non-smoking–smoking or smoking–smoking?</p> <p>References There are some errors (e.g., ref. no. 10: Gold,J and no. 12: 1967-76). You should carefully check all references.</p> |
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| REVIEWER | Jose Manuel Rodriguez Llanes Joint Research Centre, European Commission, Italy |
| REVIEW RETURNED | 07-Nov-2017 |

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| GENERAL COMMENTS | <p>Many thanks for the opportunity to review this manuscript.</p> <p>There are few available studies investigating the associations between changes in smoking prevalence and its drivers after disasters. This study is the largest of its kind and has merit. Overall (see minor changes below) the introduction and discussion are well done and the overall methodology and groups compared make sense.</p> <p>However, as presented in this version, the study overlooks important methodological aspects and at times lacks clarity. I think the authors need to pay more attention to the following major issues if they want to ensure the methodological soundness of the manuscript and effective communication of their findings:</p> <ol style="list-style-type: none"> 1) How response rates might influence the robustness of the findings – are refusals driven by sex or location (or any other available variable)? 2) A flow diagram might help in showing the data flow, data losses and the two analyses conducted 3) Show results as prevalence ratios instead of as odds ratios, as smoking is a common behavior (i.e. >10%) 4) Could the authors attach a copy of the questionnaire used? It would help to understand the work that has been done 5) Was the questionnaire piloted before being self-administered? 6) No details are given on the data entering quality control. Entering manually thousands of mailed questionnaires is an important job and typically one not exempt of challenges 7) No mention of missing variables. In most studies some questionnaire items have lower response rates than others. This point is link to point # 2 8) The tables need to be more consistent, a - including systematically the reference groups (ie, table 2), and b - adequate subheadings. I don't understand “geographical factor” as a subheading of sex and age variables. c - In the first column of table 2 the age and sex adjusted analysis seems incorrect. Each variable should be adjusted invariably by age and sex. This variation in adjustment (see *, **, ***) does not make sense and if it does, it is not explained in the methods. d - I suggest to change the reference |
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| | <p>group for age in table 2 to age (not "ages") ≥ 65. The most vulnerable seem to be the young, so it would enhance the message. e - Yet in table 2, the result for "relative's home" is OR=1.14 with 95% CI 0.73 – 0.73? There is a typo here</p> <p>9) The level of social support has been found to be an important predictor of psychological wellbeing (here consider a possible mediator of smoking) after disasters. Was this variable collected? If not, it should be acknowledged in the limitations</p> <p>10) The limitations section is too concise and requires more explanation on why each limitation matters. For instance it is better to briefly explain why a before and after questionnaire based on a single cross-section suffers from recall bias. One classical limitation is that no comparison group (non-exposed) was used in this study</p> <p>11) The conclusion does not provide any clear recommendation. Those people having a permanent house and a job did better than others. Is this supported by other studies or in relation to other variables? As such, one recommendation to the government and the affected citizens would be to prioritize these actions</p> <p>12) Informed consent is not mentioned</p> <p>13) The STROBE does not include any assessment of bias</p> <p>There are other minor corrections, which also require attention by the authors:</p> <ol style="list-style-type: none"> 1. The title is excessively long. My suggestion is to shorten it 2. Page 5 lines 8-12, consider rewording for clarity. Same for page 5 lines 35-41 3. Page 7, line 19-20 – do you mean 'remain forcefully displaced' instead of 'have still been evacuated' as of March 2017... 4. Page 7, line 46. It would be more precise to use the word 'findings' instead of "view" 5. Page 7, line 49. Replace "n" by "range" 6. Page 8, lines 8-15. Should read the prevalence of overweight, hypertension,..... 7. Page 8, lines 8-15. No verb is included – this paragraph is incomplete 8. Page 8, line 38. Do you mean 'potential mediation effect'? 9. Page 8, line 41. Do you mean 'impacts' (when the authors write 'factors') 10. Page 8, lines 52-53. "11 March 11 2011". There is problem here 11. The survey data collection process took around 10 months (jan-oct 2012). This time variation needs to be reflected when the authors discuss the findings regarding the time after the disaster. They should not use 10 months after but 10-20 months after (example first line of Discussion) 12. Page 12, line 32 – do you mean "conducted" instead of "concluded". Otherwise this short sentence is not very clear 13. Page 14, line 50 – do you mean "stratified by sex" 14. Page 15, lines 53-56 – I disagree with the sentence unless you provide confidence intervals for each estimate. If you simply look at the percentages (19.5% in 2010 and 20.7% in 2012), there seem to increase by 1.2% 15. Page 26 line 32-32. Replace "attributed" by "contributed" 16. Page 16, line 47. Why to compare with 2011 if the data for 2012 is available? 17. Page 16, line 55. "This study presents several limitations" sounds better than "the present study has certain limitations". 18. Abstract – please add briefly one sentence on why the study is needed 19. The methods explanation on the groupings in the abstract are |
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| | not clear. If you read the abstract only is not clear what was done here. |
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| REVIEWER | Dr. Barna Konkoly Thege Waypoint Centre for Mental Health Care & University of Toronto, Canada |
| REVIEW RETURNED | 10-Nov-2017 |

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| GENERAL COMMENTS | <p>This is an interesting and relevant manuscript investigating the effect of a natural disaster on smoking status (i.e. quitting and initiation of smoking). I agree with the authors that a very distinctive feature of the study is its large sample size, which is unique when investigating similar questions. I would suggest the following issues to consider when making efforts to further improve the quality of the paper:</p> <p>ABSTRACT. The authors' conclusion ('...quitting of smoking after the disaster was associated with disaster-related and psychosocial factors, especially living conditions, job status, experiences during the disaster and presence of traumatic symptoms and history of mental illness.') is in contradiction with the data they provide in the results section ('The adjusted ORs (95% CIs) for the group that quit smoking were 0.64 (0.57–0.72) for men, 1.31 (1.16–1.48) for higher education and 0.86 (0.76–0.98) for decreased income'). The conclusion statement would be true though when changing the term 'quitting of smoking' to 'change in smoking status'.</p> <p>METHODS. The authors describe that their survey included residents above 15 years of age but they excluded respondents below the age of 20 as this would be legal smoking age in Japan. However, as smoking status changes (especially initiation) more frequently in teenage years, I would strongly encourage the authors to include the population with the age range of 15-19 as well into their analyses regardless of the legal status of smoking in this age in Japan.</p> <p>I think it would be important to make it more explicit in the methodological section that only one assessment occurred (after the disaster) and so the data referencing pre-disaster characteristics are retrospective (and thus sensitive to recall biases).</p> <p>When describing the psychological assessment tools (PCL-S, Kessler Distress Scale), the authors could improve the methodical quality of their work by providing 1) exact bibliographic references regarding the original versions and the Japanese adaptations (if available) of the scales; 2) internal reliability indicators for the given sample (e.g., Cronbach's alpha).</p> <p>It would be beneficial to use the full range of information regarding some of the variables (PCL-S, distress scores, age, education), that is, not reducing the number of categories analyzed. If the authors still insist on dichotomizing their variables, they might want to give a reason to the reader for this approach (which is always waste of information compared to the original data).</p> |
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RESULTS. I would suggest the authors to use more easy-to-understand descriptors when referring to the different smoking status categories (e.g., 'quitters' instead of 'smoking–non-smoking groups').

Table 3 and 4. There is a major typo in the title of both tables: the authors use the term 'geographical' instead of '(socio)demographic'.

Table 3 and 4. Also, I found it somewhat confusing and redundant how the authors organized their analyses. More specifically, I can see no benefits of running models where all independent variables are investigated separately only controlling for 1) sex, 2) then age, 3) and then sex and age (cf. 'age and sex adjusted model' column). The note '#Adjusted for age, sex, living arrangement, experienced living in evacuation shelters, higher education, history of mental illness, unemployed, income decreased, experienced tsunami, presence of traumatic symptoms, and presence of non-specific mental illness' is also confusing to me: if these are all adjusted for, then what remained in the model? All these variables are present in reality simultaneously; therefore, it would seem to me more reasonable to run only one model for both dependent variables (quitting and initiation of smoking), which includes all independent variables simultaneously (this would also free up some space for the data from the sex-stratified analyses). And finally, even if the authors insist on the current design of the analyses, I would argue that 1) they should do the presentation of the variables consistent (now some of the independent variables are indicated to be used with covariates, while some of them not; 2) they should make it easier to follow (both here and in the statistical description of the methods section) how many models they actually ran and with which independent variables.

The authors state that the sex-stratified analyses provided 'the same' results as when using a combined sample. I would assume this means that the same variables were significant in predicting smoking status; however, I doubt that the odds ratios for example were the same as in the total sample. I would encourage the authors to report the results of the sex-stratified analyses in detail – at least as a supplementary material to their work if the word limits of the journal would not allow to incorporate those data into the core manuscript.

DISCUSSION. The authors suggest that '...to prevent people from starting smoking after a disaster, effective management of traumatic symptoms and non-specific psychological distress may be required.' I am wondering what interventions seem to be REALISTIC to use to the authors after a large scale natural disaster like the one they studied (e.g. doi:10.1002/hup.2246?). This section could be the most relevant for policy and practice; I would encourage the authors to invest more in this part of their work.

I can see an inconsistency between the two parts of the following sentence: 'The National Health Nutrition Survey reported that there

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| | <p>was <u>no change</u> in the proportions of smokers in Japan before and after the disaster: <u>19.5% in 2010 and 20.7% in 2012.</u>' Regarding their own data, the authors interpret a change with a similar magnitude as a true change. In addition, the authors continue 'A reason for the <u>slight decrease</u> in the proportion of current smokers after the disaster...' It is confusing to me why the authors speak about 'decrease' when they reported an increase just in the sentence before. Perhaps the reasons they list after that could be helpful when interpreting why the rate of smokers decreased in the studied zone of the country but then this section should be relocated to where those data are discussed (first paragraph of the discussion).</p> |
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VERSION 1 – AUTHOR RESPONSE

Responses to Reviewer1,
 We appreciate the helpful comments from the reviewer.
 Reviewer: 1
 Reviewer Name: Taishi TSUJI
 Institution and Country: Chiba University, Japan
 Competing Interests: None declared

Comment 1:

Abstract

Conclusion: Although the Authors described “The proportion of smokers decreased slightly among residents in the evacuation area”, there is no description corresponding to it in the Results section of the Abstract.

Response:

We have added the following sentence to the Abstract regarding changes in the proportion of smokers in the evacuation area:

As a result, the proportion of smokers decreased from 21.2% to 19.6% in the evacuation area.

Comment 2:

Conclusion: “the quitting of smoking after...” → “the starting of smoking after...”

Response:

We have revised the conclusion section as follows (P19 L6): The proportion of smokers decreased slightly amongst residents in the evacuation area. The changes in smoking status were associated with disaster-related psychosocial factors, particularly changes in living conditions, having experienced a tsunami, changes in jobs and post-traumatic stress disorder.

Comment 3:

Methods

P8 L17: “11 March 11 2011” → “11 March 2011”

Response:

According to the reviewer’s suggestion, we have corrected the sentence from “11 March 11 2011” → “March 11 2011” (P8 L46).

Comment 4:

Methods

P10 L13: As for the socioeconomic and disaster-related variables (i.e., living arrangement, experienced living in evacuation shelters, whether house was damaged, and whether experienced

tsunami), these look like depending on each other. Could there have been any multicollinearity effects?

Response:

Thank you very much for your comment. There was no strong collinearity amongst these socioeconomic and disaster-related variables. Therefore, we kept these variables in the multivariable adjusted analyses.

Comment 5:

Discussion

P15 L14: The related factors on changes in smoking status were not well discussed compared to discussions on the decrease in proportion of current smokers.

Furthermore, "Therefore, to prevent people from starting smoking after a disaster, effective management of traumatic symptoms and non-specific psychological distress may be required", this needs some better formulation.

Response:

We added the paragraphs shown below to the discussion to describe factors related to changes in smoking status.

Based on the reviewer's comment, "Therefore, to prevent people from starting smoking after a disaster, effective management of traumatic symptoms and non-specific psychological distress may be required", this needs some better formulation, we determined that this inference is not valid from our results. Thus, we amended the sentence as follows: "Therefore, the management of PTSD might work to prevent some individuals from initiating smoking." (P17 L6)

By contrast, previous studies have reported that the proportion of current smokers increased after a disaster: from 34.4% to 52.5% amongst men and women one year after Hurricane Katrina⁸ and from 22.6% to 23.4% amongst men and women aged ≥ 18 years 6 months after the terrorist attacks in Manhattan on September 11th, 2001.⁹

In the present study, we found that factors associated with quitting smoking were being a female, older age, having a higher education and having a stable income. Conversely, the factors associated with initiating smoking included being a male, younger age, having a lower education, staying in a rental house/apartment, experiencing damage to one's house, having experienced a tsunami, changing jobs, the presence of traumatic symptoms and non-specific psychological distress. Our results on traumatic symptoms and non-specific psychological distress were consistent with findings from previous studies on Hurricane Katrina, the September 11th terrorist attacks and the Canterbury Great Earthquake; these studies all showed that post-traumatic stress disorder (PTSD) was a major factor for initiating smoking.^{6,7,9} Therefore, the management of PTSD might help prevent some individuals from initiating smoking.

Comment 6:

Discussion

P15 L17-P16 L10: Even though the cigarette/tobacco sales significantly decreased (ref. no. 25), why the proportion of smokers in Japan slightly increased from 19.5% in 2010 to 20.7% in 2012 (rather than no change) between before and after the disaster (ref. no. 24)? Is your speculation: "these situations probably attributed to the smokers quitting smoking" true? Could you comment on this?

Response:

Damage to the tobacco industry was mainly limited to areas damaged by the earthquake, particularly Fukushima prefecture. The distribution in other regions was not affected. We have revised the corresponding sentence in the discussion section as follows (P17 L51):

One reason for the slight decrease in the proportion of current smokers in Fukushima after the disaster may be the decreased access to tobacco products. The earthquake damaged tobacco plantations, ceasing tobacco production and disrupting railway and road transportation networks in the areas damaged by the earthquake.

Comment 7:

Discussion

P17 L3: As the Authors stated in the limitation paragraph, the response rate was relatively low. Is there possibility that the proportion of smokers after the disaster was underestimated because non-respondents tend to be non-smoking–smoking or smoking–smoking?

Response:

As the reviewer pointed out, the non-respondents might include relatively more non-smoking-smoking persons compared to the respondents. Therefore, there is a possibility that the proportion of smokers after a disaster may be underestimated.

We have added a sentence about this issue in the discussion section as follows (P19 L23):

Furthermore, if non-responders tended to be smokers or beginning smokers, the proportion of smokers after the disaster could be underestimated.

Comment 8:

References

There are some errors (e.g., ref. no. 10: Gold,J and no. 12: 1967-76). You should carefully check all references.

Response:

We have corrected and changed all the references.

Responses to Reviewer2,

We appreciate the helpful comments from the reviewer.

Reviewer: 2

Reviewer Name: Jose Manuel Rodriguez Llanes

Institution and Country: Joint Research Centre, European Commission, Italy

Competing Interests: None declared

Comment 1:

How response rates might influence the robustness of the findings – are refusals driven by sex or location (or any other available variable)?

Response:

We received a similar comment from reviewer 1 (comment 7).

We have added a sentence concerning this limitation in the discussion section as follows (P19 L17):

The response rate in the present study was relatively low (41%); therefore, the representativeness of the target population was uncertain. Furthermore, if non-responders tended to be smokers or beginning smokers, the proportion of smokers after the disaster could be underestimated.

Comment 2:

A flow diagram might help in showing the data flow, data losses and the two analyses conducted

Response:

We have added a flow diagram (Figure 1).

Comment 3:

Show results as prevalence ratios instead of as odds ratios, as smoking is a common behavior (i.e. >10%)

Response:

We have revised the expression from “odds ratios” to “prevalence ratios” according to the reviewer’s advice.

Comment 4:

Could the authors attach a copy of the questionnaire used? It would help to understand the work that has been done.

Response:

We have added the URL concerning the questionnaire.

http://fukushima-mimamori.jp/foreign-languages/media/mental/01_3.pdf

Comment 5:

Was the questionnaire piloted before being self-administered?

Response:

The questionnaire was not piloted before being self-administered.

Comment 6:

No details are given on the data entering quality control. Entering manually thousands of mailed questionnaires is an important job and typically one not exempt of challenges

Response:

We used the help of experts to guarantee precision when entering the data. We also double checked all of the entered data. We added sentences concerning this issue to the methods section (P9 L37).

Comment 7:

No mention of missing variables. In most studies some questionnaire items have lower response rates than others. This point is link to point # 2

Response:

In this study, if one of smoking status, PCL and K6 was missing, these data were excluded in the analyses. The number of subjects for each variable were shown is shown in Table 1.

Comment 8:

The tables need to be more consistent,

a - including systematically the reference groups (ie, table 2), and

b - adequate subheadings. I don't understand "geographical factor" as a subheading of sex and age variables.

c - In the first column of table 2 the age and sex adjusted analysis seems incorrect.

Each variable should be adjusted invariably by age and sex. This variation in adjustment (see *, **, ***) does not make sense and if it does, it is not explained in the methods.

d - I suggest to change the reference group for age in table 2 to age (not "ages") ≥ 65 . The most vulnerable seem to be the young, so it would enhance the message.

e - Yet in table 2, the result for "relative's home" is OR=1.14 with 95% CI 0.73 – 0.73? There is a typo here

Response:

a) We have added the reference groups to the tables.

b) We have replaced 'geographical' with "sociodemographic".

c) We have revised Table 2 and added an explanation to the methods section (P12 L37).

d) We have changed the reference group for age ≥ 65 in Table 2, according to the reviewer's advice.

e) We have corrected the 95% CI.

Comment 9:

The level of social support has been found to be an important predictor of psychological wellbeing (here consider a possible mediator of smoking) after disasters. Was this variable collected? If not, it should be acknowledged in the limitations.

Response:

Thank you very much for your important comments. Unfortunately, we did not collect variables related to psychological wellbeing. However, it is difficult to estimate psychological wellbeing, and there were few existing data on the relationship between psychological wellbeing and smoking before and after a disaster. We did not acknowledge this in the limitations.

Comment 10:

The limitations section is too concise and requires more explanation on why each limitation matters. For instance it is better to briefly explain why a before and after questionnaire based on a single cross-section suffers from recall bias. One classical limitation is that no comparison group (non-exposed) was used in this study

Response:

We have revised the limitations section as follows (P19 L6):

The present study has several limitations. First, we investigated smoking status before and after the earthquake using a self-reported questionnaire administered after the disaster that involved a cross-sectional study design; this may have led to recall bias. Second, we did not assess the number of cigarettes smoked amongst current smokers, and thus we could not examine dose-response relationships. Third, because the response rate in the present study was relatively low (41%), the representativeness of target populations was uncertain. Furthermore, if non-responders tended to be smokers or beginning smokers, the proportion of smokers after the disaster could be underestimated. Lastly, there were no data from non-disaster exposed areas for comparison, except for national data on the prevalence of smoking.

Comment 11:

The conclusion does not provide any clear recommendation. Those people having a permanent house and a job did better than others. Is this supported by other studies or in relation to other variables? As such, one recommendation to the government and the affected citizens would be to prioritize these actions

Response:

A recommendation to the government is not justified by our study. We restricted the recommendation to research as described in the following paragraph (P19 L34):

In conclusion, the proportion of smokers amongst evacuees in Fukushima Prefecture decreased slightly after the disaster. The changes in smoking status after the disaster were associated with disaster-related psychosocial factors, particularly changes in living conditions, having experienced a tsunami, changes in jobs and PTSD. A long-term follow-up study is necessary to examine the effects of disaster-related factors on smoking status amongst evacuees.

Comment 12:

Informed consent is not mentioned

Response:

We have added sentences concerning informed consent in the methods section as follows (P10 L11):

The purpose of this study was explained to all responders in a cover letter distributed with the questionnaire. The cover letter clearly indicated that the return of the questionnaires would be regarded as consent for study participation. The survey data collection took 10 months (January to October of 2012), during which approximately 80% of the responses were obtained. This survey was approved by the ethics review committee of Fukushima Medical University (No. 1316).

Comment 13:

The STROBE does not include any assessment of bias

Response:

We have added the assessment of bias to STROBE.

Comment 14:

There are other minor corrections, which also require attention by the authors:

1. The title is excessively long. My suggestion is to shorten it

Response:

We have shortened the title as follows:

Effects of disaster-related and psychosocial factors on changes in smoking status after a disaster: A cross-sectional survey after the Great East Japan Earthquake

2. Page 5 lines 8-12, consider rewording for clarity. Same for page 5 lines 35-41

Response:

We have revised the sentences as follows (P5 L23):

The proportion of smokers decreased slightly amongst residents in the evacuation area. The changes in smoking status were associated with disaster-related psychosocial factors, particularly changes in living conditions, having experienced a tsunami, changes in jobs and post-traumatic stress disorder.

3. Page 7, line 19-20 – do you mean ‘remain forcefully displaced’ instead of ‘have still been evacuated’ as of March 2017...

Response:

Because some of evacuees were not forcefully displaced, we would like to remain it.

4. Page 7, line 46. It would be more precise to use the word ‘findings’ instead of “view” Response: We have made this change.

5. Page 7, line 49. Replace “n” by “range”

Response:

We have made this change.

6. Page 8, lines 8-15. Should read the prevalence of overweight, hypertension,.....

Response:

We have corrected this point (P8 L6) as the reviewer suggested: ‘Amongst the evacuees in Fukushima Prefecture, the prevalence of overweight, hypertension, diabetes mellitus, dyslipidaemia, polycythaemia and atrial fibrillation increased one year after the nuclear power plant accident.’

7. Page 8, lines 8-15. No verb is included – this paragraph is incomplete

Response:

We have corrected this point (P8 L12) as the reviewer suggested: ‘Therefore, the evacuees are expected to be at a greater risk of cardiovascular diseases if the proportion of current smoking increases because smoking is a major risk factor for cardiovascular diseases.’

8. Page 8, line 38. Do you mean ‘potential mediation effect’?

Response:

We have corrected the sentence from “potential mediation effect” to “associations” (P8 L31).

9. Page 8, line 41. Do you mean ‘impacts’ (when the authors write ‘factors’)

Response:

Thank you for your comments; we meant “impacts”, so we have revised this term (P8 L34).

10. Page 8, lines 52-53. “11 March 11 2011”. There is problem here

Response:

We have corrected the sentence from “11 March 11 2011” → “March 11 2011” according to the reviewer’s suggestion (P8 L46).

11. The survey data collection process took around 10 months (jan-oct 2012). This time variation needs to be reflected when the authors discuss the findings regarding the time after the disaster. They should not use 10 months after but 10-20 months after (example first line of Discussion)

Response:

We have added a sentence regarding this point in the methods (P10 L17):

This survey data collection took 10 months (January to October 2012), during which approximately 80% of the responses were obtained.

12. Page 12, line 32 – do you mean “conducted” instead of “concluded”. Otherwise this short sentence is not very clear

Response:

As the reviewer pointed out, “conducted” is correct. We have revised the sentence as follows (P13 L9):

Because the proportion of smokers differed between men and women, we also conducted the analyses stratified by sex.

13. Page 14, line 50 – do you mean “stratified by sex”

Response:

We meant “stratified by sex”, and we have corrected this phrase (P13 L9).

14. Page 15, lines 53-56 – I disagree with the sentence unless you provide confidence intervals for each estimate. If you simply look at the percentages (19.5% in 2010 and 20.7% in 2012), there seem to increase by 1.2%

Response:

As the reviewer pointed out, the percentage of smokers increased slightly from 2010 to 2012. We revised the sentence as follows (P17 L45):

The National Health Nutrition Survey in Japan reported that the proportion of smokers in Japan before and after the disaster increased slightly from 19.5% (95% CI, 20.0 to 20.9%) in 2010 to 20.7% (95% CI, 18.6 to 20.3%) in 2012.25

15. Page 16 line 32-32. Replace “attributed” by “contributed”

Response:

We have corrected this term according to the reviewer’s suggestion (P18 L28).

16. Page 16, line 47. Why to compare with 2011 if the data for 2012 is available?

Response:

We have corrected the sentence from “2011” to “2012” (P18 L46).

17. Page 16, line 55. “This study presents several limitations” sounds better than “the present study has certain limitations”.

Response:

We have corrected this phrase according to the reviewer’s suggestion (P19 L6).

18. Abstract – please add briefly one sentence on why the study is needed

Response:

We have added a sentence regarding this point in the Abstract:

Few studies have comprehensively examined the changes in smoking status and related factors after a disaster. We examined these factors amongst the residents of the evacuation area in Fukushima after the Great East Japan Earthquake.

19. The methods explanation on the groupings in the abstract are not clear. If you read the abstract only is not clear what was done here.

Response:

We have revised the abstract as follows:

The subjects were divided into 1) non-smokers before and after the disaster, 2) non-smokers before and smokers after the disaster, 3) smokers before and non-smokers after the disaster and 4) smokers before and after the disaster.

Responses to Reviewer3,

We appreciate the helpful comments from the reviewer.

Reviewer: 3

Reviewer Name: Dr. Barna Konkoly Thege

Institution and Country: Waypoint Centre for Mental Health Care & University of Toronto, Canada

Competing Interests: None declared

Comment 1:

ABSTRACT. The authors' conclusion ('...quitting of smoking after the disaster was associated with disaster-related and psychosocial factors, especially living conditions, job status, experiences during the disaster and presence of traumatic symptoms and history of mental illness.') is in contradiction with the data they provide in the results section ('The adjusted ORs (95% CIs) for the group that quit smoking were 0.64 (0.57–0.72) for men, 1.31 (1.16–1.48) for higher education and 0.86 (0.76–0.98) for decreased income').

The conclusion statement would be true though when changing the term 'quitting of smoking' to 'change in smoking status'

Response:

Thank you very much for your comments. We have corrected the sentence from "quitting of smoking" to "change in smoking status".

Comment 2:

METHODS. The authors describe that their survey included residents above 15 years of age but they excluded respondents below the age of 20 as this would be legal smoking age in Japan. However, as smoking status changes (especially initiation) more frequently in teenage years, I would strongly encourage the authors to include the population with the age range of 15-19 as well into their analyses regardless of the legal status of smoking in this age in Japan.

Response:

Unfortunately, the questions related to smoking status were limited to men and women 20 years of age and older; therefore, we did not have data on smoking status amongst the participants aged <20 years.

Comment 3:

I think it would be important to make it more explicit in the methodological section that only one assessment occurred (after the disaster) and so the data referencing pre-disaster characteristics are retrospective (and thus sensitive to recall biases).

Response:

We have added a sentence concerning this issue in the discussion section as a limitation (P19 L6):

First, we investigated smoking status before and after the earthquake using a self-reported questionnaire administered after the disaster that involved a cross-sectional study design; this may have led to recall bias.

Comment 4:

When describing the psychological assessment tools (PCL-S, Kessler Distress Scale), the authors could improve the methodical quality of their work by providing 1) exact bibliographic references regarding the original versions and the Japanese adaptations (if available) of the scales; 2) internal reliability indicators for the given sample (e.g., Cronbach's alpha).

Response:

We cited the references 16, 17 and 22 for the information that the reviewer requested (P24 L34).

16. Iwasa H, Suzuki Y, Shiga T, et al. Psychometric Evaluation of the Japanese Version of the Posttraumatic Stress Disorder Checklist in Community Dwellers Following the Fukushima Daiichi Nuclear Power Plant Incident-The Fukushima Health Management Survey. *SAGE Open* Jun 2016, 6 (2) 2158244016652444; DOI: 10.1177/2158244016652444

17. Suzuki Y, Yabe H, Horikoshi N, et al. Diagnostic accuracy of Japanese posttraumatic stress measures after a complex disaster: The Fukushima Health Management Survey. *Asia Pac Psychiatry*. 2016 Aug 9. doi: 10.1111/appy.12248.
22. Furukawa TA, Kawakami N, Saitoh M, et al. The performance of the Japanese version of the K6 and K10 in the World Mental Health Survey Japan. *Int J Methods Psychiatr Res* 2008;17:152–158.

These references included the following information that was not described in the test; however, we cited the references.

After the translation was authorized by the original author (Weathers et al., 1994c) of the PCL-S, a Japanese psychiatrist translated the original English version into Japanese. This Japanese version was then back-translated by two native English-speaking bilingual scientists. One scientist was a mental health professional with a background in social work and the other was a mental health professional with a background in ethnography. These two scientists were blinded to the original version of the scale. The back-translated version was then compared with the original version, and adjustments were made to the Japanese version with consideration of linguistic and semantic equivalents.¹⁶

Overall, the Japanese version of the PCL has been demonstrated to be reasonably reliable. The Cronbach's alpha of the PCL-S was 0.92 for all 17 items, 0.83 for re-experiencing, 0.82 for avoidance/numbing and 0.79 for hyperarousal. The mean score (SD) of the test-retest reliability was 42.4 (15.0) for the first test and 41.2 (15.7) for the second test, with a difference of 1.27 ($t = 0.860$, $P = 0.396$). Spearman's rank-order correlation was 0.85 ($P < 0.001$), and Spearman's rank-order correlation between the PCL and Japanese version IES-R scores was 0.90 ($P < 0.001$).¹⁷

The Japanese version of K6 demonstrated that the screening performance was essentially equivalent to that of the original English version. The performance of the two screening scales in detecting DSM-IV mood and anxiety disorders, as assessed by the area under the receiver operating characteristic curve (AUC), was excellent, with a value of 0.94 (95% confidence interval = 0.88 to 0.99) for K6.²²

Comment 5:

It would be beneficial to use the full range of information regarding some of the variables (PCLS, distress scores, age, education), that is, not reducing the number of categories analyzed. If the authors still insist on dichotomizing their variables, they might want to give a reason to the reader for this approach (which is always waste of information compared to the original data).

Response:

The PCL-S and distress scores were dichotomized because a particular cut-off point had been validated for clinical diagnosis.

The original PCL has a Cronbach's alpha of 0.939, and its correlation with the Clinician-Administered PTSD Scale for DSM-IV is 0.929. The sensitivity and specificity for a PTSD diagnosis amongst motor vehicle accident survivors or survivors of sexual assault in the United States are 0.778 and 0.864, respectively, with a cut-off point of 49/50, and 0.944 and 0.864, respectively, with a cut-off point of 43/44.¹⁶

The K6/K10 scales were reported to detect major depression and dysthymia most effectively according to the DSM-IV. Kessler recommended using a cut-off score of 12/13 as an indicator of severe mental illness.

Age was not dichotomized in the present study. We categorized age into three groups with a sufficient number of participants. We categorized the academic record using a cut-off of 14/15 years of education. This cut-off point conformed to the guidelines of the International Standard Classification of Education, ISCED level 5B (<http://uis.unesco.org/sites/default/files/documents/international-standard-classification-of-education-isced-2011-en.pdf> (accessed 22 Dec 2017)).

Comment 6:

RESULTS. I would suggest the authors to use more easy-to-understand descriptors when referring to the different smoking status categories (e.g., 'quitters' instead of 'smoking–nonsmoking groups').

Response:

We have revised the smoking status categories as follows:

'smoking–non-smoking groups' to 'quitters'

'non-smoking–smoking groups' to 'starters'

'smoking-smoking group' to 'smokers'

'non-smoking–non-smoking group' to 'non-smokers'

Comment 7:

Table 3 and 4. There is a major typo in the title of both tables: the authors use the term 'geographical' instead of '(socio)demographic'.

Response:

We have replaced 'geographical' with "demographic" in the tables.

Comment 8:

Table 3 and 4. Also, I found it somewhat confusing and redundant how the authors organized their analyses. More specifically, I can see no benefits of running models where all independent variables are investigated separately only controlling for 1) sex, 2) then age, 3) and then sex and age (cf. 'age and sex adjusted model' column).

The note '#Adjusted for age, sex, living arrangement, experienced living in evacuation shelters, higher education, history of mental illness, unemployed, income decreased, experienced tsunami, presence of traumatic symptoms, and presence of non-specific mental illness' is also confusing to me: if these are all adjusted for, then what remained in the model?

All these variables are present in reality simultaneously; therefore, it would seem to me more reasonable to run only one model for both dependent variables (quitting and initiation of smoking), which includes all independent variables simultaneously (this would also free up some space for the data from the sex-stratified analyses).

And finally, even if the authors insist on the current design of the analyses, I would argue that 1) they should do the presentation of the variables consistent (now some of the independent variables are indicated to be used with covariates, while some of them not; 2) they should make it easier to follow (both here and in the statistical description of the methods section) how many models they actually ran and with which independent variables.

When we exact association between education level and prevalence ratios of quite smoking. We regarded other factor as compared and their adjusted psychosocial variables.

Response:

Thank you for your thoughtful comment. We changed the strategy for the multiple adjustment. First, we adjusted for age and sex. Then, in the multivariable models, we only used the statistically significant variables. In the multivariable models, we adjusted the variables except for the variables of interest, which served as the covariates for the adjustment. We corrected Tables 2 and 3 and described this strategy in the methods (P14 L31).

We added Supplemental Tables 1 and 2, which contain sex-specific results, to the multivariable models.

Comment 9:

The authors state that the sex-stratified analyses provided 'the same' results as when using a combined sample. I would assume this means that the same variables were significant in predicting smoking status; however, I doubt that the odds ratios for example were the same as in the total

sample. I would encourage the authors to report the results of the sex-stratified analyses in detail – at least as a supplementary material to their work if the word limits of the journal would not allow to incorporate those data into the core manuscript.

Response:

As we mentioned above, we added the supplemental tables and show the results stratified by sex.

Comment 10:

DISCUSSION. The authors suggest that ‘...to prevent people from starting smoking after a disaster, effective management of traumatic symptoms and non-specific psychological distress may be required.’ I am wondering what interventions seem to be REALISTIC to use to the authors after a large scale natural disaster like the one they studied (e.g. doi:10.1002/hup.2246?). This section could be the most relevant for policy and practice; I would encourage the authors to invest more in this part of their work.

Response:

We deleted the discussion on policy and practice because it was beyond our results.

Comment 11:

I can see an inconsistency between the two parts of the following sentence: ‘The National Health Nutrition Survey reported that there was no change in the proportions of smokers in Japan before and after the disaster: 19.5% in 2010 and 20.7% in 2012.’ Regarding their own data, the authors interpret a change with a similar magnitude as a true change. In addition, the authors continue ‘A reason for the slight decrease in the proportion of current smokers after the disaster...’ It is confusing to me why the authors speak about ‘decrease’ when they reported an increase just in the sentence before. Perhaps the reasons they list after that could be helpful when interpreting why the rate of smokers decreased in the studied zone of the country but then this section should be relocated to where those data are discussed (first paragraph of the discussion).

Response:

We apologized that our expressions related to smoking status confused you. We have revised the paragraph in the discussion as follows (P17 L46):

The National Health Nutrition Survey in Japan reported that the proportion of smokers in Japan before and after the disaster increased slightly from 19.5% (95% CI, 20.0 to 20.9%) in 2010 to 20.7% (95% CI, 18.6 to 20.3%) in 2012.²⁵ One reason for the slight decrease in the proportion of current smokers in Fukushima after the disaster may be the decreased access to tobacco products. The earthquake damaged tobacco plantations, ceasing tobacco production and disrupting railway and road transportation networks in the areas damaged by the earthquake. Tobacco distribution stagnated after the disaster. According to a press release by Japan Tobacco Inc., the total cigarette sales volume in April 2011 decreased by 81.8% compared to April 2010, and domestic cigarette sales decreased by 74.8%. Furthermore, the cigarette sales volume between April and September 2011 decreased by 41.2% and the tobacco sales volume decreased by 20.4% compared to the same period the previous year.²⁶ These situations likely contributed to quitting smoking after the disaster in Fukushima.

VERSION 2 – REVIEW

| | |
|------------------------|---|
| REVIEWER | Taishi TSUJI Chiba University, Japan |
| REVIEW RETURNED | 31-Jan-2018 |

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|-------------------------|--|
| GENERAL COMMENTS | It seems that the manuscript was revised properly. |
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|-----------------|---|
| REVIEWER | Jose Manuel Rodriguez Llanes European Commission, Joint Research Centre, Italy |
|-----------------|---|

| | |
|-------------------------|--|
| REVIEW RETURNED | 08-Feb-2018 |
| GENERAL COMMENTS | <p>Thanks for the opportunity to revise this article again.</p> <p>I found the authors did a good job improving the manuscript. Overall, this is a good addition to the literature. I still think the English requires improvement to be ready for publication. Please find in the attached document some English issues that I spotted (not exhaustive but for illustration of the remaining issues).</p> <p>- The reviewer provided a marked copy with additional comments. Please contact the publisher for full details.</p> |

VERSION 2 – AUTHOR RESPONSE

Responses to Editorial Requests

We appreciate the helpful comments from the editor.

Comment 1:

- We agree with reviewer 2 about the quality of English, which is not at the requisite standard for publication in places. Can you please thoroughly proofread the paper one more time? We recommend consulting a native English speaker if possible.

Examples:

1. Table 3 title includes a typo: “disaster-related and psychosotial factors..”

2. There is also a typo in the table itself: “Psychosociall factors”

- In the previous decision letter reviewer 3 also points out that “geographical” should be “demographic” in the title of table 3. This does not appear to be different in this revision. Please clarify.

- Regarding the following comment from reviewer 3 and your response in the rebuttal letter:

Response:

We asked a native English speaker to proof the paper thoroughly one more time.

According to the editor's suggestion, we have corrected the sentence from "Psychosocial factors" to "Psychosocial factors" (Table1, Table2, Table3)

Comment 2:

METHODS. The authors describe that their survey included residents above 15 years of age but they excluded respondents below the age of 20 as this would be legal smoking age in Japan. However, as smoking status changes (especially initiation) more frequently in teenage years, I would strongly encourage the authors to include the population with the age range of 15-19 as well into their analyses regardless of the legal status of smoking in this age in Japan.

Response:

Unfortunately, the questions related to smoking status were limited to men and women 20 years of age and older; therefore, we did not have data on smoking status amongst the participants aged <20 years.

From the Editor: Can you please highlight the above in the limitations section of the manuscript?

Response:

We apologise that we did not add the sentence in the limitation section in the previous version. We have added a sentence concerning this limitation in the discussion section as follows. (P30 L7)

Because smoking status changes more frequently in the teenage years, psychosocial factors may influence smoking status, particularly in teenagers. Unfortunately, the questions associated with smoking status were limited to men and women 20 years of age and older in the present study. Therefore, we did not evaluate an association between psychosocial factors and smoking status amongst the participants aged <20 years.

Comment 3:

3. You have described this as a cross-sectional study but you use "cause and effect" language in your manuscript (e.g. in the title). Cross-sectional studies can only examine associations, not causes and effects. Can you please revise your language or justify why you are referring to "effects"?

Response:

As the editor pointed out, our study is a cross-sectional study. However, we asked the participants about changes in smoking status before and after the earthquake, and causality between disaster-

related factors and smoking status is assumable because smoking status could not lead a natural disaster. We have changed the expressions from “effect” to “association” throughout the entire text, according to the editor’s advice.

We have revised the title as follows.

Associations of disaster-related and psychosocial factors with changes in smoking status after a disaster: A cross-sectional survey after the Great East Japan Earthquake

According to the editor’s suggestion, we have corrected the sentence from “impact” to “association” (P14 L23, P14, L29)

According to the editor’s suggestion, we have corrected the sentence from “related factors” to “associated factors” (P14 L42, P14, L48)

Comment 4:

- The ‘Strengths and Limitation’ should have a total of at least three (3) and a maximum number of five (5) bullet format. Please amend accordingly.

Response:

We have corrected it.

Comment 5:

-Authors must include a statement in the Methods section of the manuscript under the sub-heading ‘Patient and Public Involvement’.

This should provide a brief response to the following questions:

-How was the development of the research question and outcome measures informed by patients’ priorities, experience, and preferences?

-How did you involve patients in the design of this study?

-Were patients involved in the recruitment to and conduct of the study?

-How will the results be disseminated to study participants?

-For randomised controlled trials, was the burden of the intervention assessed by patients themselves?

-Patient advisers should also be thanked in the contributorship statement/acknowledgements.

If patients and or public were not involved please state this.

Response:

Patients and public were not involved in the present study.

We added explanation concerning patient consent in the manuscript as follows:

Patient consent:

Not required.

Responses to Reviewer1,

Reviewer Name: Taishi TSUJI

Institution and Country: Chiba University, Japan

Competing Interests: None declared

It seems that the manuscript was revised properly.

Response:

Thank you very much for your comment.

Responses to Reviewer2,

We appreciate the helpful comments from the reviewer.

Reviewer: 2

Reviewer Name: Jose Manuel Rodriguez Llanes

Institution and Country: European Commission, Joint Research Centre, Italy

Competing Interests: None declared.

Thanks for the opportunity to revise this article again.

I found the authors did a good job improving the manuscript. Overall, this is a good addition to the literature. I still think the English requires improvement to be ready for publication. Please find in the attached document some English issues that I spotted (not exhaustive but for illustration of the remaining issues).

Response:

Thank you very much for your kind assistance. We asked a native English speaker to proof the paper thoroughly one more time.

Comment 1:

"the" should be replaced by "a"

Response:

We have corrected the sentence from "Using the self-administered" to "Using a self-administered". (P5 L20)

Comment 2:

take out "the"

Response:

We have corrected it. (P5 L22)

Comment 3:

a "," is needed after "education"

Response:

We added "a" after "education". (P6 L9)

Comment 4:

do the authors mean displaced?

Response:

Most of them were still evacuated, so we would like to keep the expression. (P8 L19)

Comment 5:

Here you need a "." instead of ","

Response:

We corrected it. (P6 L9)

Comment 6:

"Change in jobs" do sound odd, better "change jobs"

Response:

We have revised the expression from "change in jobs" to "change jobs" throughout the text. (Table1, Table2, Table3, Supplement table1, Supplement table2, P6L14, P6L31, P11L6, P13L51,P20L28, P21L12, P21L45, P27L46, P30L17)

Comment 7:

Correct "The" to "the".

Response:

We have corrected it. (P30 L9)

Comment 8:

"a" or "the"

Response:

We have corrected it. (P30 L14)

Comment:

switching jobs

Response:

We have corrected it. (P31 L17)

VERSION 3 – REVIEW

| | |
|-------------------------|--|
| REVIEWER | Jose M Rodriguez-Llanes European Commission, Joint Research Centre, Italy |
| REVIEW RETURNED | 20-Apr-2018 |
| GENERAL COMMENTS | Many thanks to the authors for taking my text corrections into consideration. I also noticed by reading the manuscript again that the English has improved. This seems to me ready for publication. |

