Author's response to reviews

Title: Factors associated with good self-rated health of non-disabled elderly living alone in Japan: a cross-sectional study

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Version: 2 Date: 7 September 2007

Author's response to reviews: see over
Dear Senior Assistant Editor Iratxe Puebla:

Thanks a lot for those comments on my paper (MS: 2434520851296770). I learned a lot from the comments. I revised my paper as the reviewers advised and submitted the revised manuscript.

Now, I would like to give a point-by-point response to the comments.

Answer to reviewer Maria Victoria Zunzunegui:

Major Compulsory Revisions:

Q1. Title “characteristics that contribute” should be changed to “Factors associated with”...

Answer: As the reviewer advised, I changed the title into “Factors associated with good self-rated health of non-disabled elderly living alone in Japan: a cross-sectional study”.

Q2. Abstract

More information on response rates of the survey, variables included, statistical analysis should be given. Conclusions are a repetition of results.

Answer: The response rate of the survey was added in page2, line14: “Response rate was 75.1%”.

As for the variables included, I had added all variables into the abstract. However, there is limitation for words count in the abstract: <=350 words. If I added all the variables, the word count will over 380 even if I tried to adjust. Now, the abstract is 347 words, very near to 350. I felt very sorry for this point. Please kindly understand me, if possible.

As for statistical analysis, I added “Multivariate logistic regression was used to identify the factors associated with good SRH and sex-specific effect was test by stepwise logistic regression (page2, line16-17)”.

Conclusion is also changed like “For the non-disabled elderly living alone, sex-appropriate support should be considered by health promotion system from the view point of SRH. Overall, the ability to go out alone to distant places is crucial to SRH of both men and women (page3, line5-7)”.

Q3. Introduction

1) An estimate of the percentage of elderly who live alone in Japan should be given, and if possible, this figure should be given separately by sex.

Answer: I added this information in page4, line15-16.
“Until 2005, Japanese Statistics Bureau reported that over 15% of elderly (9.7% of men, and 19.0% of women) lived alone (Figure 1).” And the figure was added in the paper.

2) There is a lack of conceptual framework. The absence of socioeconomic position indicators is hard to justify since these are known correlated of SRH in most population. 

**Answer:** I added more reference papers to show the factors associated with SRH and the socioeconomic status was added (page4, line6-12). As for our study, the Law on Personal Information Protection has been implemented in Japan since April 1st, 2005, whereas this study was conducted in October 2005. We could not access to socioeconomic information. However, every elderly Japanese can get pension regardless whether they worked or not when they were young. We explained this in detail in Method section (page7, line16~). We also regretted that we could assess this status if we began to perform this study a little earlier.

3) A list of anticipated results would be useful to understand the choice of predictors in the model.

**Answer:** This question, together with the question that the other reviewer asked me—why I focused on the limited factors, made me decide to add a section about “factor selection for the assessment” in Method (page7, line13~). So, I just added a list of the variables that we selected to assess in this study (page5, line 11~) in the Introduction. I do not know whether this change is suitable or not. I am very grateful for your advice if you think I need make further change.

**Q4. Method:**

1) More detail should be given on choice of categorization of variables: depression, risk of fall, and IADL.

**Answer:** As for depression, this cut-off point was used as the diagnosis of depression. I added it in page11, line6-7: “Score $\geq 2$ was used to diagnose the presence of depressive symptoms, as reported by Rinaldi P [28]”. As for risk of fall, it was used frequently as “fall risk” and “no fall risk” in Japan. So, we decided to use the same standard (page9, line20 - page10, line5). As for IADL, we made sure again with previous report, especially the reported conducted in Japanese population. We found that we confused a little before, so we are very thankful for your advice. Now, we corrected this definition and categorization (page10, line14-18).

2) Statistical analysis
Answer: sex*risk factors were test and the results were added. Also, stepwise was replaced by purposeful selection as you advised. However, for assessing the sex-specific effects, stepwise was still used because the risk factors already were identified. The detail is in page11, line17-page12, line17.

Q5. Results: a table with the overall mode and interaction terms is needed.
Answer: Table 3 and 4 were added to show these results. Also, the chi-square analysis in total population was also added to Table 2.

Minor Essential Revisions:
Q1. Is age treated as continuous in the final model?
Answer: Age is treated as continuous when we perform the logistic regression. “Age was treated as continuous variable” was added in the footnote of tables.

Answer to reviewer Shahin Shooshtari:
Major Compulsory Revisions
Q1. a) Expand on the Background Section of the manuscript to have a more in-depth discussion of broad range of factors that affect health and self-rated health among the older population.
Answer: I added some reference paper about the factors in relative to SRH among the elderly in Background section (page4, line6~).

b) To explain (in the Methods Section) why they have focused on a limited number of indicators.
Answer: I added “factor selection for the assessment” (page7, line13~) in the Method section. I am very thankful if you have more advices.

c) Discuss the limitations of their study in the Discussion Section.
Answer: I listed out the limitation of our study in Discussion. The detail is in page17, line10~.

Q2. Cognitive impairment and dementia should be clearly explained. Chronic
disease should be discussed about it limitation in Discussion section.

**Answer:** Our subjects do not include the person who has cognitive impairment (page6, line10). In Japan, The Welfare Commission is a system for providing support for the elderly living alone. This organization knew the condition of elderly living alone very well. If the elderly has cognitive impairment, he/she will be given the special healthcare in another organization or begins to use long-term health insurance. To clarify this, I explained the function of Welfare Commissioner in “study population” of Method (page6, line15–).

As for chronic disease, our data were reported by the study subjects themselves. So, I redefined the chronic disease as “self-rated chronic disease” through the whole paper. And I wrote the limitation for this item in Discussion (page17, line12–).

**Q3. It is difficult to consider “regular habit of walking” as one of the indicators measuring social activity.**

**Answer:** Thanks a lot for this question. We really confused this before. This item should belong to lifestyle factor. We actually wanted to use it to indicate the physical activity. In order to clear its meaning, I changed it into “having a walk regularly” through the whole paper. As for the lifestyle factors, we actually also assessed the alcohol consumption. About all these changes and the reason why we did not showed alcohol consumption in the data, I wrote in page8, line14-19; page10, line18-page11, line3; page11, line18-20; and page17, line14-15.

**Q4. What are the response categories for social network? The reference supporting the definition should be given.**

**Answer:** In order to support this definition, I cited a reference (page11, line13-15).

**Q5. Statistical analysis.**

**Answer:** I changed this section. In addition, as advised by other reviewer, I used multivariate logistic regression and stepwise logistic regression to assess the risk factors and sex-specific effects, as written in page11, line17–.

Minor essential revision:

**Q1. On page 5, to clarify community-dwelling.**

**Answer:** I changed the original sentence into “most of SRH studies have focused on the elderly who lives in the community regardless of living arrangement” (page5, line4-5).
Q2. The accuracy of the translation of SRH.
Answer: I checked the accuracy of this item. It does like what I had translated. Before we conducted this survey, we had checked the reference papers about SRH and found that this version is used more often than other type. Especially, a survey conducted in Japanese population also used this version and dichotomized the response. So, we decided to use the same method and translated from English into Japanese. I am sure that it was done like this.

Q3. On page 5, authors presented total number of residents aged 65 or older. They need to cite the source for that data.
Answer: We got this data from the Welfare Commission. The data there is different from the national registration system. So, I could not cite the source of this data. But I give the detail about “definition of non-disable elderly living alone” in Method section (page5, line20~) and “study population” (page6, line15).

Q4. There is no information on the validity and reliability of the questionnaire. The author need to provide more information on the development of the survey questionnaire itself, its content and validity and reliability of the questions used.
Answer: Since this is the first attempt in Japan, we mainly referred the previous study. The factors that have been reported to correlate with SRH were assessed in this study. Also, we considered the specialty of “living alone” (page7, line13~). We did not assess the validity. As for reliability, the two-week test-retest reliability was performed. I wrote it like this: “Two-week reliability of the questionnaire was 90.3%” (page9, line10-11). For the validity, we will be very appreciated if you want to give us more advices.

Q5. The authors need clearly explain why they have dichotomized the study outcome. The explanation provided on page6 is not sufficient.
Answer: I mainly referred a study conducted in the Japanese population: “With referring the study conducted in middle-age Japanese [7]” page7, line8.

Q6. Authors need to explain what are instrumental ADL as one of the eight items to measure physical health.
Answer: Instrumental ADL was defined wrong in my previous manuscript. I checked it again and corrected it. Thanks a lot! The change is in page10, line14-18.

Q7. Chronic disease.
**Answer:** Chronic disease is mainly reported by subjects themselves. I changed it into a more accurate term “self-rated chronic disease”. It mainly means “any disease including hypertension, cardiovascular disease, diabetes, or stroke had been diagnosed” page10, line12-14.

**Q8. Why weight loss had been considered as …..**

**Answer:** The reason why we considered weight loss is “weight loss, especially unintentional weight loss, is common among the elderly and can reflect disease severity or undiagnosed illness, as reviewed by Alibhai SM [19]. Weight loss is also included in the assessment.” (page8, line10-13). As for the standard “5kg in the past 6 months”, I added a reference for it: “for which we referred the criteria usually used in nutritional assessment in Japan [25]” page10, line11-12.

These changes are made with considering reviewers’ comments. If the change is not sufficient, we are looking forward for further advices! Thanks in advance!

Best wishes,
Sincerely

Wei Sun