Author's response to reviews

Title: Social participation and mental health: Moderating effects of gender, social role and rurality

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Version: 2 Date: 21 June 2013

Author's response to reviews: see over
We very much appreciate the comments made by our reviewers. We have made the following specific changes to the manuscript in response to their helpful suggestions.

**Reviewer #1’s comments:**

#1

Many of the measures used were categorical but were replaced by the midpoint of these categories and then used as continuous variables with calculating means. Also in the regression model this is problematic since it assumes a linear association on a logarithmic scale. Instead, these variables should be treated as categorical in the analyses.

Thank you for your suggestion. We have treated age, equivalised annual income and years of education (that were previously used as continuous variables) as categorical variables in the regression analyses. As the new table shows, the effects of main independent variables of this study (i.e. the main effects of social participation and social role as well as the interaction effect) did not change from the previous table.
The principal component analysis variable is difficult to interpret. If the authors cannot explain it clearly (what is for example social participation -0.10 in Figure 1?), some other combination of the different types of participation should be used.

The first principal component score is just the weighted total sum of social participation.
items. We can judge, from the factor loadings and communality scores derived from the principal component analysis (Table 1), that the simple sum of each dummy variable for social participation (measured as 1 or 0) is not appropriate for the analyses. For example, from Table 1, the communality score of religious group is low. This means that participation in religious group is less correlated with other types of participation. Thus, treating participation in religious groups and civic/consumer movement groups (which shows the highest loading score) equally within the same scale would be inappropriate. Therefore, the first principal component score used in this study is a weighted sum of 8 types of social participation.

The social participation variable was centered on the mean. Thus -0.10 on Figure 1 means -.10 standard deviations below the mean. We added the term “centered on the mean” in the Statistical Analysis section (page 10, line 10).

#3

The authors should also be more explicit why multilevel analysis was used and what additional information it brought into the study. Cannot living in urban/rural area be treated as an individual level variable?

It is assumed that the outcome variable among respondents who were sampled from the same school district would be clustered, i.e. display a positive intraclass correlation. In this case, the independence of observations (one of the assumptions in OLS regression) is violated and the appropriate standard error cannot be estimated. The reason for using the multilevel model is to estimate the robust standard error adjusting for intraclass correlation. We think this is an essential analysis when the data structure is nested (or clustered), as in the present study.
How many of the respondents were excluded because they had depressive symptoms at baseline? Can this have affected the results?

1,650 respondents (21.0%) were omitted because of depressive symptoms at baseline (Other omitted respondents were due to missing values). Because the aim of this study is to analyze the new onset of depressive symptoms between the first wave and second wave survey, we excluded respondents who already reported depressive symptoms at baseline. This is important because previous studies of social participation and depression may have suffered from “reverse causation”, i.e. depressive symptoms result in lower social participation (not the other way round). By excluding prevalent depression at baseline, we avoid this bias.

The discussion section needs to be developed. In the current form it is just a repetition of the main results, with some speculations of possible explanations based on Japanese society and one limitation. The results of the current study should be discussed in the context of similar studies carried out in Japan and elsewhere. The comments on special features of Japanese society should be justified by references. The limitations of the study should be more thoroughly concerned, the validity of the measures used and the generalizability of the results should be addressed. For example, can it be a problem that the results are based only about 10% of the original sample? The public health consequences of the findings about the differences in the effects of social participation on depressive symptoms among the aged in Japan, and possibly elsewhere, should also be discussed.
Thank you for your comments. We have added references of studies that help to contextualize our results in the field.

“Thus, this result is consistent with Kavanagh’s [8] and Norton et al.’s [10] arguments that women receive more benefit from their social participation than men.” (page 12, line 22)

“In contrast to Kawachi and Berkman’s [4] argument that frequent social participation may bring about psychological distress for women, this suggests that social participation which provides the individual with a social role does not adversely impact the mental health of women, and may even promote the mental health of men”. (page 13, line 4)

“Our results offer new insight into health promotion among the elderly, and shed light on the interaction between the social context and social participation.” (page 14, line 5)

Next, regarding the problem of deletion of a number of respondents, the aim of this study is to capture the new onset of depressive symptoms between first wave and second wave survey and to explain it by the preceding data (the first wave survey data). As mentioned previously, this is important because previous studies of social participation and depression may have suffered from “reverse causation”, i.e. depressive symptoms result in lower social participation (not the other way round). By excluding prevalent depression at baseline, we avoid this bias.

Thirdly, the special features of Japanese society described in this paper are anecdotally reported hypotheses. To our knowledge, there is no international-standard research that investigates the closed nature of social networks, the effects of sanction, and the presence of intolerance in Japanese rural communities. Thus, the interpretation of the findings from the present study is hypothetical and intended to pose questions for further investigation.

We have added the following sentence in last paragraph as a part of limitation (page 14, line 16).
“In addition, the characteristics of social network (tightly bonded) in Japanese rural areas described in this paper are speculative, and require further ethnographic elaboration.”

Fourth, for the public health consequences, we have added the additional sentence suggesting this point.

“The present study’s suggestion that it is important to have roles in social participation for retired men is critical for developing community-based interventions to promote the health of elders.” (page 13, line 15)

#6

Table 2 should be commented in the results section.

We have added the following sentences at the beginning of Results section (page 10, line 21).

“For the percentage of depressive symptoms, we found no major difference between men and women. For equivalised annual income, the proportion of respondents who have an income less than 150 million yen is slightly higher for women than for men. In addition the percentage of respondents with 6 or fewer years of education is also slightly higher for women than for men. The proportion of respondents who are married and living with a spouse is higher for men than for women. The proportion of respondents who report playing a key role in organizations is also higher for men than for women.”

#7

Abstract: The conclusions are too diffuse to be useful, please be more specific.

We have changed the conclusion in abstract as follows.

“Our findings support the notion that increasing the opportunities for social participation
improves older people’s health, especially for women. However, in the rural Japanese context, offering men meaningful roles within organizations may be important.”

#8
Table 3 is referred as Table 1 in the text (pages 10-11).

Thank you for pointing that out. We have corrected “Table 1” to “Table 3” (page 11, line 9; page 11, line 20).

#9
The term “physical activity” should be deleted from page 3 since it is not addressed anywhere else in the manuscript.

According to the comment, we have deleted the term “physical activity” from this sentence (page 3, second line of Background section).

Reviewer #2’s comments:

#1
The background literature review mainly references older research papers, the most recent being from 2006. Are there any more recent relevant papers in relation to gender, location and older people's social participation that could be cited? If not, this is important to point out, that this paper is addressing a gap where there has been a lack of research, and a gap that has not been addressed in recent studies. This could also be emphasised in the discussion.

Thank you for your suggestion. To our knowledge, there is no research that examines the interaction effect of older people’s social participation, role in the organizations, and the
urban/rural context of the community. Thus, we have added a sentence that emphasizes this point in the Background section (page 5, line 24).

“To our knowledge, there is little research that examines the interaction effect of older people’s social participation, role in organizations, and the urban/rural context of community on their mental health.”

#2
Are there any studies or references that can be cited to support the authors’ description of social networks (tightly bonded) in Japanese rural society (at the end of the background section)?

The special features of Japanese society (tightly bonded social networks) described in this paper are anecdotally reported hypotheses. To our knowledge, there is no international-standard research that investigates the closed nature of social networks, the effects of sanction, and the presence of intolerance in Japanese rural communities. Thus, the interpretation of the findings from the present study is hypothetical and intended to pose questions for further investigation. We have added the following sentence in last paragraph as a part of limitation (page 14, line 16).

“In addition, the characteristics of social network (tightly bonded) in Japanese rural areas described in this paper are speculative, and require further ethnographic elaboration.”

#3
In the discussion, the authors largely summarise their 3 notable findings without providing references to contextualise their research with the broader literature. Is each of their main findings consistent with the results of any previous studies? If not, this is important to state, in terms of the significant new contribution this paper makes. Are the conclusions - ie. about the
nature of social networks in urban and rural areas - supported broadly by any previous studies?

Thank you for your suggestion. We have added some explanation with some studies that contextualize our results in Discussion section.

“Thus, this result is consistent with Kavanagh’s [8] and Norton et al.’s [10] arguments that women receive more benefit from their social participation than men.” (page 12, line 22)

“In contrast to Kawachi and Berkman’s [4] argument that frequent social participation may bring about psychological distress for women, this suggests that social participation which provides the individual with a social role does not adversely impact the mental health of women, and may even promote the mental health of men. (page 13, line 4)

“Our results offer new insight into health promotion among the elderly, and shed light on the interaction between the social context and social participation.” (page 14, line 5)

#4
In relation to using proportion of workers in school district who are employed in agriculture/primary industry as an indicator of rurality, has this been done before in Japanese research?

Yes. For example Hanibuchi (2008) used the proportion of primary industrial workers as one of indices of rurality. We have modified the explanation of the neighborhood-level variable in Methods section as follows (page 9, line 25).

“Thus, in order to create a cross-level interaction term, we used the school district-level proportion of workers engaged in primary industry (agriculture), following Hanibuchi [16] who used the primary industrial workers rate as one of indicators of rurality.”
In terms of the demographic variables (household income, marital status etc) entered into the analysis from responses from the first wave of the survey - how did you consider whether some of these may have changed between the first and second wave - ie. if a spouse had died in that time, or household income had changed. Would this make a difference to the overall analysis? It would be good to clarify this.

Thank you for your suggestion. This study used only independent variables that robustly precede outcome (onset of depressive symptoms) in terms of time. Although we agree that death of spouse significantly affects the development of respondents’ depressive symptoms, we did not use this variable because date of spouse’s death and onset of depressive symptoms cannot be identified. Even if the death of partner and change of household income have significant impact on the respondents’ mental health, they are not main hypotheses of this study. In addition, we think including these variables in the regression models does not change the estimation of the effects of our main independent variables (sex, social participation, role, and rurality).

In the results section, in the sixth paragraph (beginning with 'Fourth') - it appears the wrong table is referred to - it should be table 3 and not table 1.

We have altered “Table 1” to “Table 3”. (page 11, line 9; page 11, line 20)

In the following paragraph describing the figures, it is not clear in the the figure titles or in the text that they are separately referring to men and women.
We have added figure number in places where each figure is referred (page 12, line 10; page 12, line 15).

We would like to thank the reviewers again for their helpful suggestions. We believe that our paper is improved as a result of attending to their suggestions, and we hope that our paper is now acceptable for publication. We look forward to hearing from you.

Sincerely