Reviewer’s report

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

Version: 1 Date: 26 April 2013

Reviewer: Anu Molarius

Reviewer’s report:

The manuscript is based on a prospective study investigating whether the effects of social participation on depressive symptoms differ between men and women, between different organizational positions or between urban/rural settings among the aged in Japan.

The topic is interesting, the material seems sound and the introduction is well written. But there are a number of unclarities concerning the analyses performed and the interpretation of the findings, and the discussion requires a major revision.

Major Compulsory Revisions

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.

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 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?

How many of the respondents were excluded because they had depressive symptoms at baseline? Can this have affected the results?

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.

Table 2 should be commented in the results section.

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

Minor Essential Revisions
Table 3 is referred as Table 1 in the text (pages 10-11).

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

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.