Reviewer's report

Title: The effects of mid-life socioeconomic disadvantage and perceived social support on trajectories of late-life depressive symptoms among older Taiwanese women

Version: 1 Date: 29 January 2014

Reviewer: Hal Kendig

Reviewers report:

This article addresses an important topic area for which there are few longitudinal research findings available generally and virtually nothing on ageing in Chinese cultural contexts. The Taiwan Longitudinal Survey is a very good data base well suited for these analyses. The substantive concerns for midlife effects of socio-economic circumstances on mental health outcomes are important. These potential contributions indicate the value of the work if improvements can be made as suggested below:

- Major Compulsory Revisions.

1. An empirical article such as this one requires clear hypotheses or at least specified research questions developed in the context of the relevant literature. There is a broad reference here to what is termed the life course hypothesis (page 1, para 2) but this work by Blane and others is best termed a perspective or a framework. Hypotheses testing would bring this work beyond description and provide a critical focus. Blane’s work derives from a broader life course epidemiology literature (eg Mishra) that warrants attention.

2. The article begins with a broad and interesting literature review which with more focus could lead up to the key analytical questions (hypotheses). I suggest that published work on life span predictors of quality of life and mental health, for example, recent work by David Blane, James Nazroo, and Paul Higgs are places to start. I believe that there is longitudinal work through mid and later life on social support and a good place to start here is the work of Tony Antonucci. While the CESD measures have been applied in non Western cultures, it would help to include some reference to the cross-cultural appropriateness and validation of the methods.

3. Given the life span focus the article requires at least brief information on the social/historical context of Taiwan during the periods in which the survey participants have lived their lives. A life course framework requires information on the socio-economic ‘exposures’ during which the respondents have lived the (potentially) critical periods of their lives. French et al. published in this journal 2012 show how the societal context can be taken into account in a cross-cultural analysis. What has been the ‘social positions’ of these women as they have
grown older in a rapidly developing Taiwan in which women’s status has also been changing? This information is important for the global literature in itself, as well as for context in this article.

4. The growth modelling statistical approach appears to be broadly appropriate and potentially is strength of the article. However, the rationale for why this technique is being applied needs clarification, along with explication of the specific procedures, measures and the time points for their use in building the modelling. For example, was economic strain (and for that matter all other variables) measured at each observation point and with each observation entered in the analyses (and if so how?) It would help to refer to and follow an authoritative publication demonstrating a related application of the technique to socio-economic modelling of life course data.

5. Table 1 should be expanded (possibly in two tables) to show how the characteristics of the sample have changed over each of the study observation periods (and these patterns require description and interpretation). Table 2 would benefit from more explicit description of the results in the text.

6. It is surprising to see that SES is apparently taken into account only in terms of the women’s own positions with no account being taken of the social positions of partners of those who are in a relationship. The SES position of a couple could be indicated by the highest occupation of either of the partners. If information is not available on the partners’ SES, it will be important to state this as a significant limitation of the study.

7. The discussion requires a critical interpretation of the findings in the context of the relevant literature (as requested above). For example, on p 12 line 7 there is reference to the ‘accumulation’ effect. This reads as an aside comment when it would be much better to have an interpretation in terms of the core life span epidemiology concepts. For example, of accumulation and critical period effects. The statement that life time ‘major occupation had no significant impact’ is surprising and requires care to make sure that the measure is not mis-specified (see above). Does the article need a different title if SES does not matter? Was any modelling done to see if there were any critical thresholds or interactions in SES effects? The comment on p13line4 commented that ‘social support was assumed to be constant’. This was surprising to see at this late point in the article; this proposition critical to the article’s contribution required (and could be) analysed in this data set rather than having a vague reference to earlier literature.

8. The conclusions are broadly appropriate – reflecting the potential of this research – but their credibility depends on a revised article addressing the queries in the above points. This article has the potential to make these valuable contributions to the literature and therefore in my estimation further careful work would be richly important for the field.

9. The writing would benefit from careful editing to ensure precision of expression in English and to reduce the scope for cultural misinterpretation. For example the running header of ‘Women’s distress’ appears to be odd given the content of the article.
10. Given the strong credentials of the authors in health promotion, it would be valuable if they would develop in the discussion their views on the implications of the findings for investment in social capital over the life span. For example, do the findings suggest that we should be investing early in life because it is too late later in life?

11. Given rapid social change do the authors have any comments on how these patterns observed for the older cohorts now might apply to the ‘life trajectories’ of those who will become old in the future? It is beyond the scope of this article but the data would lend themselves to a class ‘cross-sequential design that would shed important light on these questions. This might be mentioned as a direction for further research.

- Discretionary Revisions

Figure 1 presents admirably good response rates but I wonder if this information is not available from general descriptions of the survey. A specific question: was socio-economic status or social support associated with differential non response?

Otherwise none beyond those implied in the required revisions above.

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

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests’