Author's response to reviews

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

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Author's response to reviews: see over
Dear Editor:

Enclosed is a revised research article entitled "The effects of mid-life socioeconomic disadvantage and perceived social support on trajectories of late-life depressive symptoms among older Taiwanese women" (MS: 1814645066113273), which we are resubmitting for possible publication in BMC Public Health. We would like to thank you and the reviewers for the constructive comments on our previously manuscript. This current version has been revised in response to the points raised by the reviews. Our response to each enumerated concern from the Editor and the reviewers is given below.

Reviewer #1:
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:

REPLY: We would like to thank the reviewer for the suggestions and have revised our manuscript as suggested.

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.

REPLY: As suggested by the Reviewer, we revised the background section to present the life course hypothesis (Pg. 3-4). Then, we also demonstrated hypotheses testing supported by the relevant literature of suggest (Pg. 5).

Additional references were added:


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 CES-D 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.

**REPLY:** As suggested, we revised the background to present the issue of life span predictors in mental health by introducing published work by Wiggins et al (2007) and Antonucci et al (2002) for (Pg. 3).

Additional references were added:


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 societial 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.

**REPLY:** As suggested, we have provided background context about Taiwanese
women and its possible relationship with SES and emotional distress among women (Pg. 5).

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.

**REPLY:** As suggested we have provided the rationale for growth curve models and specific modeling strategies with an authoritative publication (Pg. 9-10).

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.

**REPLY:** As suggested, we added the descriptions about the sample characteristics across waves (Pg. 12). In addition, we have also plotted figures (Figure 2 and 3) that took Reviewers’ suggestions into account. In Figure 2, the predicted trajectories of depressive symptomatology on two domains without covariates were presented. In Figure 2, the predicted trajectories were further stratified by education and economic strain (Pg. 28-29).

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.

**REPLY:** In separate analyses, we have included education and main occupation of women’s partners as suggested. Similar major findings were obtained: the SES of the partners did not produce an independent effect on women’s depressive symptomatology. Yet, this analysis brought another important issue in relative effects of women’s own position vs. partners’ position on trajectories of women’s depressive symptomatology. We have acknowledged in the Discussion section of the manuscript. Using the results and additional analyses of this study, the next logical step will be to assess the role of relative SES effects of women and their
partners, which may be life-course in nature (Pg. 15).

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 p.13 line4 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.

REPLY: As suggested, we have added additional interpretations on accumulation effect and related modification for occupation has been made accordingly (Pg. 15). As prior research suggested (Lynch and Kaplan 2000), various social variables compose individual socioeconomic status. Such variables as education and occupation were hypothesized to influence individual health but through different social pathways. We thus consider various dimensions of SES.

In addition to previous research on estimation of social support, we also conducted analyses that indicated stability of perceived social support. We have added aforementioned information to the manuscript (Pg. 10).

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.

REPLY: We would like to thank the reviewer for the suggestions and have revised our manuscript as suggested.

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.

REPLY: We would like to thank the reviewer. We have corrected typographical and
grammar errors as well as the running header.

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?

**REPLY:** We have added the discussion on the implications of our findings for investment in social capital over the life span as suggested (Pg. 16).

Additional references were added:

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.

**REPLY:** We have added this suggested direction (Pg. 18-19).

**Discretionary Revisions:**

12. 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 socioeconomic status or social support associated with differential non response?

**REPLY:** Response rates are available from general descriptions of the surveys. We have added attrition analyses that assessed differences in mid-life socioeconomic disadvantage and social support between continuing participants and those participations that were lost to follow-up (Pg. 7).

Reviewer #2:
I enjoyed reading “The effects of mid-life socioeconomic disadvantage and perceived social support on trajectories of late-life depressive symptoms among older Taiwanese
women,” a well-written manuscript. The research question is well-defined, cites appropriate literature, the data seem appropriate to answer the question, and the abstract and title convey what is found. There are some concerns about the analysis. Below are suggested revisions to improve upon the manuscript:

**REPLY:** We would like to thank the reviewer for the suggestions and have revised our manuscript as suggested.

**Discretionary Revisions:**
1) Classifying the trajectories as “late life” is a misnomer for some women if they are 50 at baseline and followed for only 12 years. Consider revising.

**REPLY:** As suggested, we have revised "late life" to "subsequent" in title and abstract and throughout the text.

**Minor Essential Revisions:**
1) Some more discussion of the implication of these results for future research in Taiwan and other populations (and both men and women) would situate this particular analysis with other research.

**REPLY:** As suggested, we have added more discussion of the implication of these results for future research (Pg. 15, 18-19).

**Major Compulsory Revisions:**
1) One issue with growth curve models in general that is concerning is that the covariates of interest (socioeconomic disadvantage and perceived social support) are treated as time-invariant (accounting for their measurement at midlife) but are actually time-varying covariates. If the data do not contain repeated measures of these covariates, then the model cannot treat them as time-varying. If this is the case, some discussion of potential model misspecification seems to be in order, as socioeconomic disadvantage and perceived social support in theory may vary over time in relation to depression trajectories in a way that is not accounted for here.

**REPLY:** We agree that SES and perceived social support are time-variant measures and changes of in SES and social support are likely to have a substantial influence on subsequent depressive symptomatology as women age. However, based on the prior literature, we believe it was necessary to first clarify the cumulative association between SES and social support in mid-life and trajectories of depressive symptoms over time. To truly be able to understand this complex relationship, we used a simplified life-course perspective to assess this temporal relationship, and therefore SES and social support were based on mid-life measures. Conceptualizing
these variables as time-variant is an important issue for future research now acknowledged in the Discussion section of the manuscript. Using the results of this study, the next logical step would be to assess the role of SES and social support, which are time-varying in nature (Pg. 10, 18).

Additional reference was added:

2) The manuscript discusses “cumulative effects.” The literature on cumulative disadvantage and inequality is rather broad and disparate, and it is unclear which sort of accumulation the authors are seeking to address.

REPLY: We take a life-course perspective and define cumulative disadvantage effects as accumulating SES disadvantage across the life course as Blane (2006) suggested. Mid-life SES disadvantage is thus hypothesized to be associated with not only onset of but changes related to subsequent depressive symptomatology (Pg. 3-4).

3) The methods need to be clearly explicated in the manuscript. There are many ways to specify growth curve models, and it should be clear in the manuscript beyond providing a citation which methods were used.

REPLY: In order to assess how the trajectories of depressive symptomatology vary with mid-life SES and social support, the present analysis employed two-level growth curve models in which the models were specified with age at Level 1, and nested within individuals at Level 2. The models estimated with random coefficients. The random-coefficient models explored the relationships between depressive symptomatology and mid-life SES and social support by specifying a person-specific random intercept and a person-specific random slope for these two mid-life predictors. We have added the above interpretation to the Method and Result Sections (Pg. 9-10, 12).

4) Similarly, it is not clear on page 6 why there are two distinct factor domains of depressive symptomatology, especially since one domain only has two indicators. Some evidence that these are distinct domains in these data rather than separating because other studies have is warranted.

REPLY: Our detailed description of the psychometric analysis of the 2 domains for the 10-item CES-D in the TLSA study has been published in a previous work. In response to the comment of Reviewer, we have added a reference to the CES-D
items of two domains to the measures section (Pg. 7).

5) In addition, it appears that the slope of each trajectory is not statistically significant. How is the reader to interpret this finding?

**REPLY:** The slope of each trajectory represents the effects of mid-life covariates on subsequent changes of depressive symptomatology over time. In our analyses, we have found a significant effect of mid-life education on the change over time among women with 7-9 years of education in the lack of positive affect ($\beta = -0.05$, p <0.05) (Pg. 13). These women had a significantly steeper decrease in level of symptoms in the lack of positive affect than those who had no formal education as they became olders (Pg. 13). Our findings demonstrate a significant and profound effect of education on depressive symptomatology among a subgroup of women. We have discussed the above findings in the Discussion Section (Pg. 14-15).

6) Table 2 is hard to understand if one is not familiar with growth curves. Explanation of the terms *mean growth* would aid in interpretation. (Also, “affect” rather than “affects” in the labels.)

**REPLY:** Thanks to the Reviewer for careful checks. We have corrected “affects” to “affect” in the table labels (Pg. 28). We have also provided an explanation for the term ”*mean growth” in the Method and Results Sections (Pg. 9-10, 12).

7) Figures would be important to display the results more substantively. In particular, figures that plot the trajectories of negative affect and lack of positive affect with no covariates, and each of the covariates that significantly impacted the intercept and slope of the trajectory.

**REPLY:** As suggested, the predicted trajectories with no covariates and stratified by education and economic strain in depressive symptomatology were presented in Figures 2 and 3 (Pg. 30-31).

8) Again, for readers that are not familiar with growth curves, using the same language with respect to changes over time (such as “rate of change”) would be useful.

**REPLY:** As suggested, we have revised “rate of change” to “changes over time” (Pg. 11-13).

9) Page 13: the discussion of why measures of physical health were not included is unclear. It seems like physical health should be accounted for in this analysis if the data
REPLY: Exclusion of the physical health measure was necessary because there was little variation in its measurement across the sample. In our growth curve model, change is modeled on the basis of aging that takes physical decline into consideration. In addition, we have separately run the models with physical health measures as suggested. Similar major findings were obtained. We have added the above interpretation to the Discussion Section (Pg 16-17).