Reviewer's report

Title: When does poor subjective financial position hurt the elderly? Testing the interaction with educational attainment using a national representative longitudinal survey

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Reviewer: Peter Smith

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When does poor subjective financial position hurt the elderly? Testing the interaction with educational attainment using a national representative longitudinal survey

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The objective of this paper was to explore the interaction between financial position and education with respect to three health measures: self-rated health, depression and mortality, among an elderly Taiwanese population. I agree with the authors that explorations of possible interactions between perceived socioeconomic position and objective measures of socioeconomic status are important to undertake, as are examinations of the predictors of poor health and mortality in the elderly. However, I do have some substantial reservations about the article in its current form, which I have outlined below.

Major Compulsory Revisions

1. More information is needed about the study sample, non-response and attrition. In the data section we are not even told the number of respondents (although it is presented in the abstract). The authors have directed us to a 1989 report for more information about the sample, however, they should provide some minimal information so that readers (and reviewers) do not have to go in search of this report. Specifically we need to know. Who was the target population for the initial survey? What was the response rate to this survey? If this survey was initiated in 1989, who dropped out of the sample between 1989 and 1996 (the first year that the authors have used)? As the authors have only used respondents with complete data we also need to know what percent of respondents had missing data? Which questions were more likely to be missing? Was the missing-ness random or was it associated with some of the main independent or dependent variables. Without this information we know very little about how the generalise these results. In addition, the authors have told us it is a “sample of elderly persons”. What was the age range of the respondents?

2. More information needs to be provided about the modeling strategy. The authors have used a GEE model to assess the relationships between education
and financial strain with two or their three outcomes. First, more information is needed about the model. For example, was education used as a time-varying factor or a fixed factor? There appears to be some movement for increasing education (which is surprising given the age of the respondents) over the time periods, but it is minimal. I would also like the authors to justify why they have chosen a GEE model. My understanding of this modeling technique is that it allows us to examine how variation in one variable (e.g. education) impacts on variation in another (e.g. self-rated health). However, in their introduction the authors have presented a desire to examine the causal relationship between measures of socioeconomic position and health. However, the GEE model will not allow us to determine the causal sequence of measures of socioeconomic status (e.g. financial strain) on health, but only that when financial strain increases health deteriorates. However, this could be that when health deteriorates this brings about an increased perceived financial strain (e.g. due to future and current health care and support needs). This is less of an issue for education. However, if education is a fixed factor, then why use a GEE model in the first place. In short, I would have liked to authors to examine if their conclusions remain robust to different modeling approaches (e.g. does financial strain at baseline predict worsening self-rated health, or movement to depression at three or seven years)?

3. The collapsing of the self-rated health and financial strain seem fairly arbitrary. There appears to be sufficient sample to keep satisfied and very satisfied for financial status separate. We know that with job satisfaction (for example) being very satisfied and being satisfied represent very different things. Similarly with self-rated health I'm perplexed as to why the bottom three categories have been collapsed to be the outcome of interest, given they constitute about 70% of the sample. Have the authors considered using different cut points, or keeping self-rated health as an ordered scale. Again it would be good to report on these sensitivity analyses to know that the results are robust to different specifications of the data.

4. I would have appreciated the authors presenting the results of the interaction analysis. For example, we would expect that those who are financially dissatisfied and in the lowest educational group to have the highest probability of each outcome, given they have two risk factors (low education and high financial strain). The interaction is testing whether the impact of financial strain differs across level of education. So, I would have presented the results of financial strain across level of education (rather than education across financial strain), and presented to odds ratios within educational strata, so that we can assess if being dissatisfied financially has a greater impact (relative to being very satisfied) among lower educational groups than among higher educational groups. It would be also useful – in interpreting the confidence bounds – to know the relative size of each of the education by financial strain groups.

Minor Essential Revisions

1. I would encourage the authors to be more explicit about what are measures of perceived socioeconomic position and what are self-reported measures of
socioeconomic position in their introduction. For example, in the last section of their introduction they list a series of measures labelled as “subjective financial position”. However, some of these are relatively objective questions (e.g. do you have any money left at the end of the month). Self-reported information is often criticised as being more susceptible to various sources of bias, than other more “objective” sources of data (e.g. through negative or positive affectivity, acquiescence or social desirability; item priming effects). However, just because something is self-reported does not make it necessarily associated with self-reporting biases as listed above. I think the distinction that the authors want to make is that two people with the same level of self-reported income (for example), may have different perceived levels of financial strain, given expectations, comparison groups etc..

2. In the methods section under socioeconomic status measures the authors state that “Educational attainment and the respondent’s major occupation were used as measures of objective SES”. However, analyses using occupation are never presented. I would encourage the authors to go with only education, as occupation is also illogical under the same grounds as removing income, given the population is assumed to be retired. Therefore they should delete occupation from this section.

3. The interpretation of the mortality results by the authors were a little perplexing. One, there is a clear U shaped gradient in mortality across education level, which is not mentioned at all, and the authors state that financial strain is not associated with mortality, however the results in table to present a statistically significant elevated risk of mortality among those who are dissatisfied with their financial position.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests