Reviewer's report

Title: Explanations of socioeconomic differences in changes in physical function in older adults: results from the Longitudinal Aging Study Amsterdam

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Reviewer: James S House

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Major Compulsory Revisions
This paper analyzes an important problem using a generally appropriate and good set of data. Although it is definitely improved from the prior version, it still lacks a fully clear and consistent focus. The original intended focus of the paper, as reflected in the introduction, was to examine the contribution of disease, behavioral, and psychological factors to explaining socioeconomic differences in changes of physical function among person 55 and over in the Longitudinal Aging Study Amsterdam (LASA). However, the empirical focus of the paper is equally or more that there are socioeconomic differences in trajectories of physical health for persons 55-70 but not for those aged 70+, an issue which only is tacked on as the last sentence of the introduction without any prior or subsequent conceptual development.

In many ways the paper can and does deal more adequately with the latter issue than the former, and there has developed a substantial literature over the past two decades on the issue of whether socioeconomic disparities in health are relatively constant or more variable by age, with at least some of that literature consistent with what is found here: socioeconomic disparities in health increase over most of the adult life span through middle and early old age and then converge (see papers and books by House et al, Lynch, Mirowsky and Ross, Smith, West, and others for a range of views and evidence on this issue).

I think the paper would be more effectively focused if it made the age difference in socioeconomic disparities its primary or equal focus throughout (e.g., in the intro as well as the methods, results, and discussion), which is the way the background section of the abstract is now stated. This would require bringing in more conceptual and empirical literature and discussion re the age differences to better balance these with the data and the other focus on explanatory factors. As noted below it also requires more clearly recognizing the strengths of the study for analyzing these age differences and its weaknesses for explaining them and the socioeconomic disparities (or lack thereof).

Minor Essential Revisions
1. More attention should be paid to potential problems due to nonresponse (NR). The original NESTOR study of living arrangements had only a 62% response rate, and the LASA recruited only 82% of these yielding an overall baseline response rate of 51% relative to the target population, with further panel attrition. Greater discussion and analyses of the potential impact or bias due to this NR is needed.
2. The description of the income variable on p. 5 is confusing. It appears that the measure being used is really an estimate of individual income, not net household income as it is described. Even accounting for this, the levels seem perhaps a little low, so I also wonder what is meant by net income. A case could be made for using only the total household income (regardless of number of persons in household), so some discussion of whether this would make any difference is also desirable.
3. On p. 5 and elsewhere, different ways of handling missing data across different variables are described: some is imputed based on other variables/information, some is set to the mean, some is simply coded as missing using a dummy, and sometimes cases with missing data are excluded. Generally imputation based on other variables/information and/or coding missing cases via a dummy is preferable for independent variables, and imputation and/or excluding missing cases for dependent variables. The authors should have a more consistent and defensible approach throughout the paper and across variables, and should note whether results are robust to different approaches.
4. The description of the multilevel modeling on p. 7 is clearer than in the prior version, but could be more so. Presumably there is a level 1 model which just estimates a growth curve for each person as a function of time, and then a series of level 2 models which estimate the effects of other variables on the person-specific intercepts and slopes derived from the level 1 model. It is not quite clear to me what is meant by between age and sex and longitudinal time. Are these not just estimates of the effects of age and sex in the level 2 model predicting the slopes and level 1? Perhaps I am having some trouble translating what the SPSS software, which I do not know, does into what HLM or SAS software, that I am more familiar with, does. In any case, the authors need to make the nature of the models clearer.
5. Since the finding of an interaction between age and SES in predicting the longitudinal time slopes or growth curves structures the entire rest of the results, it should be described more fully in the methods and
results (and motivated more in the introduction as noted under major compulsory revisions). Was the only interaction test done using a dummy for SS-69 vs. 70+ or were other categorical or continuous versions of age also explored?

6. Again, I am having trouble understanding exactly where the numbers in Tables 3 and 4 come from, though they appear quite consistent with the descriptive data in Table 2. I think these are the actual coefficients for the SES variables in the level 2 models, which indicate how the level 1 intercepts and slopes vary by SES. Again the use of the SES longitudinal time terminology is confusing. It appears that the education and income effects are being estimated in separate models rather than in a single multivariate model, but this is not fully clear.

7. Again what is meant by the interactions between explanatory factors and longitudinal time is not fully clear. Are these cross-level interactions? And what do they mean conceptually?

8. I think the discussion will be improved by attending to the issues noted under major compulsory revisions. Most of the current discussion is quite speculative and unrelated to the data. (For example, selection effects are adduced on p. 11, but in no way documented in the data, and the literature suggests multiple reasons that SES effects/disparities may be weakened as health at older age). On p. 12 and in the conclusion, suggestions are made about differences in onset vs. progression of disease and the role of behavioral vs. psychosocial factors in them, but all this is based on associations of factors with baseline levels, which are really cross-sectional and hence causally ambiguous. In fact the study finds little or nothing except SES that predicts change (growth curve slopes), and speculates on many reasons for this, some of which (e.g., handling of NR and missing data) are addressable as noted above. In my view, a paper more focused on the age differences in ways the SES predicts health declines provides clearer and stronger data, and would provide the basis for discussing how and why the reasons for this could and should be better explored in the future.

Discretionary Revisions

1. The description of the education categories is not fully clear or self-explanatory for a reader not conversant with Dutch/European education systems. Some translation into American equivalents and/or approximate years of formal schooling could help.

2. On p. 6, it would be useful to note if other scalings of the physical function were tried, and whether they make any difference.

3. Given that the respondents all had a medical exam, some discussion of the rationale and validity of using self-report measures of disease would be useful.

4. On p. 7, it could be made clearer that you have separate measures of instrumental and social emotional support, as becomes clear in Table 1.

5. Sentences toward the bottom of p. 8 might be better worded as follows:

a. In both age groups, there were more nonsmokers and fewer nondrinkers in the lowest SES groups (p<.01).

b. In both groups, this association (with physical activity) was present for education, but not for income.