Author's response to reviews

Title: Bias and heteroscedastic memory error in self-reported health behavior: an investigation using covariance structure analysis

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PDF covering letter
Reply to the reviewers

Reviewer 1: J Scott Long

1. First, the outcome variables (number of partners at various time periods) are count variables. There are convincing reasons for not using the multiple regression models for count outcomes (see for example Cameron and Trivedi, Regression Analysis of Count Data, Cambridge, 1998). The multiple regression model is a special case of the CSA model used in this paper. Accordingly, there are problems with applying the CSA to count variables. Cameron and Trivedi, Chapter 10, discuss issues of measurement related to count outcomes, and I believe that there has been more recent work in this area.

Another approach is to consider the more general model proposed by Bengt Muthen and estimated by the program Mplus which allows outcome variables that are measured on an ordinal scale.

Applicability of covariance structure analysis (CSA) to count data may indeed be limited if these are heavy tailed or generally speaking non-normally distributed. This is a well placed concern with the data analyzed and was briefly discussed in the paper. However, the limitations are due to the maximum likelihood (ML) estimation of the standard errors of the parameters and some goodness of fit indices which assume multivariate normality rather than to some intrinsic properties of the count data (these can be normally distributed, although this is not often the case). There are two basic strategies to deal with serious violations of multivariate normality assumption in regression:

a) transform the data to reduce kurtosis and linearize the relationships between the variables of interest, then proceed with CSA using estimation methods relying on multivariate normality such as ML or least squares, or

b) use methods which do not assume multivariate normality, such as asymptotic distribution free (ADF) estimation first proposed by M. Browne in 1984 and developed further by Bentler and Yuan (among others), rank-based methods such as the work of Muthen & Muthen cited above, bootstrapping, or the method proposed by Cameron & Trivedi cited above.

This paper used both strategies (cube root transformation and Browne’s ADF), although it did not exhaust all the techniques within the two as this was not the purpose of this work. In addition, a range of model fit indices were used to test various aspects of the model. All these elements indicated a good fit of the CSA model to the count data at hand. This empirical evidence shows that the model works reasonably well for these data. Finally, the discussion in the paper concentrated on the data at hand without extrapolating to some other data and caution was recommended not to confound the analysis presented with a validation based on independent evidence from other data addressing the same topic.

It is important to underline the need to analyze the count data in medical research. A Poisson regression is a standard statistical approach based on logarithmic transformation of
the counts. Cube root transformation worked better with these data but the logic is still that of the generalized linear models – the most widely used statistical modeling technique of our time.

The paper already discussed some of the above issues in ‘Transformations and estimation method’. This has now been expanded and commented in the discussion, including the work of Cameron & Trivedi and Muthen & Muthen cited above.

2. Second, the major problem I have with the analysis is that all conclusions on measurement error in the counts are contingent upon other parts of the model (especially the instrumental variables) to identify the degree of error, since there is only a single indicator of each outcome variable.

The model of interest cannot have but a single indicator of each outcome variable as this is a basic decomposition of self-reported observed variables into true and error components. As in any regression model, the error estimation is contingent upon structural model parameters. In the CSA model presented, the instrumental variables play a crucial role in predicting ‘true’ values of self-reported outcomes. The model residuals were relied upon to check how successful this strategy was. In addition, it was stressed in discussion that the interpretation of the model presented should be that of a possible and plausible scenario compatible with the data analyzed.

3. The choice of instrumental variables selected to affect each outcome, shown in Figure 1, appears to be largely ad hoc. For example, why would current smoking affect partners in the period greater than five years? Or, being with parents until 16 affect only recent partners but not those further into the past (at which point the respondent would be closer to the period of influence from the parents)? Consequently, I am not convinced by the conclusions.

On the contrary to the concern expressed above, the instrumental variables were carefully chosen on the basis of published research as stated in the paper (a reference was also given). This has now been emphasized in the new version of the paper and another reference was added. For example, current smoking was associated with larger number of sexual partners in various studies, two of which were based on the same survey (Johnson et al., 1994; Kupek, 1999). This effect may be stronger during adolescence and early adulthood, which corresponds to the period of >5 years ago for most of the subjects analyzed in this work. Not living with both natural parents until 16 years of age was associated with having more sexual partners only for the most recent period reported, possibly due to the strong effects of other predictors such as the age of sexual debut and the carry-over effect of the number of partners from the earlier period.

Other plausible theoretical explanations for these and other effects in the model may be found but I wish to stress that an empirical rather than a purely theoretical basis guided the instrumental variables’ selection. This was already stated and referenced but has now been underlined. The choice of focusing on the error part rather than on the structural relationship in the CSA model presented is now stated explicitly. Notwithstanding the importance of the latter both for estimating the error components and in its own right, the
aims of this paper never included analysis of specific contributions of a wide range of social and psychological factors influencing the number of sexual partners.

Reviewer 2. Isaac Dialsingh

Discretionary revisions
1. The abstract should be properly written. I believe the abstract should state the problem and define it properly- this was done in the background section.

I am not sure what changes are suggested but suppose some elaboration of the problem was recommended, so I added a sentence in the background paragraph of the abstract. The rest of the abstract follows the obligatory structure of the journal, i.e. methods, results, conclusions.

2. The figures and tables should not be at the end of the paper. They should accompany the appropriate data analysis discussion.

Following the advice of the editors, the figures and the tables should remain at the end of the paper.

3. The paper is basically the application of a statistics tool - structural equation modeling but no reference to this is used in the paper.

My preference is for the term ‘covariance structure analysis’ and the reasons for it are now explained in the text. However, ‘structural equation modeling’ has now been added to the terms used for this technique.

4. The writing style is acceptable and the writer supplies enough details both on the methodology and the analysis for even the amateur reader to get a sufficient grasp of the information.

I conclude no changes in the writing style are required then.

Reviewer 3: Paul J Hewson

Compulsory revisions:
1. The model specification as given in equations 4.1, 4.2, 4.3 and 4.4 is consistent with the model results given in Table 3. However, it is difficult to follow these parameters and variables through to the model diagram given in Figure 1. It may be better to use consistent nomenclature in Figure 1.
The difficulty arises from the complexity of the model and I am afraid there is no simple remedy for it. The nomenclature in Figure 1 may seem inconsistent to the reader because the subscript logic was not explicit for all of them, although their basic principles were defined in ‘Model specification’ (first paragraph). This has now been briefly explained in the results when Figure 1 is first mentioned (last sentence in the first paragraph under ‘Covariance structure model’ subtitle).

2. There are some extremely minor typographical errors which need correcting, for example, under "Materials and Methods" "Subjects" in the fifth paragraph we have "All man aged 26 - 35 years".

These have been corrected.

Discretionary revisions:
1. The overall focus of the paper is not entirely clear. Is it an exposition of a potentially useful technique which would be of interest to researchers in a broader range of fields, or is it intended as an addition to the literature on the "Sexual Attitudes and Lifestyles Survey"?

The overall focus is on a potentially useful technique which would be of interest to the researchers dealing with self-reported behavior amenable to memory error in general. As health behavior is increasingly researched in this way, an illustrative example was provided with nationally representative data from the survey cited (see the last sentence in the first paragraph of the Background). In addition, the title tries to capture both the general methodological issue and the specific research topic used for illustration, thus making it potentially interesting for a wider audience.

2. The introduction of equations 1 and 2 in the "Statistical Methods" section is rather abrupt for a non-technical audience any may benefit from a clearer exposition. There is little detail given under "Transformations and estimation method" as to how Cubic root transformation has been applied, and why this transformation was chosen. Likewise, some of the more technical comments in the discussion are not explained well, for example it is not clear how "generalised inverse of a Hessian Matrix" may be a simple solution to collinearity problems.

The equations 1 and 2 are a common way of formalizing the CSA model fitting. A warning to the reader about the technical nature of the exposition to follow is now in place. Technical details on how to deal with collinearity were omitted on purpose to avoid overloading the text with technical details of secondary importance here but a reference was provided for those interested. Similar treatment was given to the issues of transformation and estimation methods. As for the cubic root transformation, kurtosis reduction was already said to be the motivation for it. I tried to restate this more clearly in the new version of the paper.