Sherry Handfinger Reviewer #4

From: <Drjmy3@aol.com>
To: <handfing@email.unc.edu>
Sent: Tuesday, August 30, 2005 2:12 PM
Subject: Norbeck Social Support manuscript

Sherry,

I've just spent a couple hours trying to figure out what the author(s) did in the analysis and understanding the front part.

The "background" section is still very difficult to follow. Perhaps an "insider" to this research can understand it easily, but that will not be most of the readers. There are few if any transitions and no "road signs" for the reader. So, it is difficult to follow and seems like it is just jumping from topic to topic. On p. 6, the paragraph that starts, "Referring to the six original NSSQ items in table 1..." is especially confusing. I don't see (or agree) that "How much does this person help you to relax or re-energize arter work?" is an indicator of "long-term aid" as asserted in that paragraph. This is the first mention of Norbeck's two-factor structure and the two-factor structure (the names of the the two factors) is never identified. The last sentence is confusing because the stuff after the semi-colon doesn't seem to go with the stuff before it. Again as in the previous draft (as I remember), in the next paragraph, the author(s) warn agains combining the situation-specific items with the original items, but that is EXACTLY what they do in the analysis.

The Methods section has a number of paragraphs that are copied directly (sometimes with changed numbers) from Gagliotti's 2002 paper in NR (see page 279). The copied paragraphs are the first two under "Procedure" and the first paragraph under "Data Analysis." Maybe that is ok since it is the same journal, but thought you might want to check that.

***My biggest concern is that the testing of the alternate model is not correct. Specifically, it is not possible to have a Chi Square of 0 and a df of 0 unless the model is just identified. If that is the case, it will ALWAYS fit exactly because the number of parameters to be estimated and the amount of information supplied to the program to be analyzed is the same. So the author(s) cannot talk about the perfect fit as a great new finding. It is a matter of fact for all models where this is the case. Indeed, one has to have at least 1 df in order to get a p value. The fact that they got a negative error variance (not covariance as stated??) usually means that the model is mis-specified.

In addition, I think from the description that the alternate model only includes the three situation specific items where the first model used all six items (3 original and 3 situation specific). It is not appropriate to compare these two models. They are apples and oranges. In the previous review, I was thinking that they should test either a 1-factor model with all 6 items, or test a two factor model -- one factor is the original items and one factor is the situation specific items -- since that is what they imply in the background section (I think!).

The conclusions don't really go with the findings.

I would advise that you have a real statistician look at the analysis section. It is a very technical presentation (which I think is a mistake for most readers) and there are some statements that I think are not correct, at least I couldn't find them in the book the author(s) reference.

JoAnne

Sherry Handfinger

From: "Sherry Handfinger" <handfing@email.unc.edu>
To: "Eileen Gigliotti" <gigliotti@mail.csi.cuny.edu>
Sent: Monday, January 09, 2006 4:10 PM
Attach: Duplicate publication article by Broome.pdf; Duplicate publication article by von Elm.pdf; Reviewer4 & 5 comments only_05_027.pdf; 2005_027_Gigliotti copyedited.doc
Subject: Re: Manuscript # 2005/027- reviewer and editor comments

Dear Dr. Gigliotti:

Earlier (email of October 21, 2005) I let you know that questions were raised by one of the reviewers on your manuscript and that we would likely need to have some changes in your manuscript.

Attached are additional comments by the original reviewer and the comments of a reviewer who had all of the reviews and other materials related to your manuscript plus a copyedited version of your manuscript. Please use this copy to make your revisions and return it to Sherry at handfing@email.unc.edu.

Please address the comments of these reviewers and make changes as called for in the manuscript. Focus on Reviewer #5 as a guide to the changes that are needed, but do not neglect the consensus of Reviewer #4.

I am concerned about duplicate publication. Please review (a) Broome, ME (2004). Self-plagiarism: Oxymoron, fair use or scientific misconduct. *Nursing Outlook*, 52(6): 273-4 and (b) von Elm, E, Poglia, Walder, B, & Tramer, MR (2005). Different patterns of duplicate publication: An analysis of articles used in systematic reviews. *JAMA*, 291(8), 974-980 (see attachments) and make any modifications needed to avert duplicate publication.

Sincerely,

Molly C. Dougherty, PhD, FAAN
Editor

Sherry Handfinger Ms. has been accepted —

From: "Eileen Gigliotti" <gigliotti@mail.csi.cuny.edu>
To: "Sherry Handfinger" <handfing@email.unc.edu>
Sent: Tuesday, January 24, 2006 11:45 AM
Attach: copyedited[2].doc
Subject: Re: Manuscript # 2005/027- reviewer and editor comments

Dear Dr. Dougherty,
I am attaching my revised copy of manuscript 2005-027. I found the reviewers' comments very helpful . I made the following changes and highlighted these on the manuscript:
Reviewer 5
1. Title was changed to reflect CFA and not "uses"
2. Associate Professor was changed to Professor to reflect my new rank as of January 1st 2006.
3. All references to measurement error reduction or consideration was changed to reflect validity focus
4. Three models were evaluated as suggested by reviewer 5. Note that only correlated models were evaluated because the NSSQ is a correlated factors measure and this was demonstrated in my 2002 article.
5. All six items were evaluated in all models. Multivariate skew and kurtosis were provided in the text and Table 3 was combined with Table 4. Univariate skew and kurtosis were provided in table 3.
6. The correlation matrix was reordered.
7. due to ordinal variables and nonnormality GWLS was used. In order to use this method, an asymptotic (large sample) covariance matrix was computed through Prelis 2. This is what is suggested by Joreskog and Sorbom (1996). This is highlighted in the text.
8. An explicit model selection approach was taken as suggested.

Reviewer 4
9. I reworded the paragraphs under procedure and data analysis which were replicas of my 2002 article.
10. All six items were evaluated in all models.
11. I reworked the results and conclusions sections as suggested and these are highlighted.

Thank you for your attention to this revision and I look forward to your response.
Eileen Gigliotti

---------- Original Message ----------
From: "Sherry Handfinger" <handfing@email.unc.edu>
Date: Mon, 9 Jan 2006 16:10:53 -0500

>Dear Dr. Gigliotti:
>
>Earlier (email of October 21, 2005) I let you know that questions were raised by one of the reviewers on your manuscript and that we would likely need to have some changes in your manuscript.
>
>Attached are additional comments by the original reviewer and the comments of a reviewer who had

Sherry Handfinger

From: "Sherry Handfinger" <handfing@email.unc.edu>
To: "Eileen Gigliotti" <gigliotti@mail.csi.cuny.edu>
Sent: Monday, February 06, 2006 3:27 PM
Subject: Re: Clarification regarding manuscript #2005/027 accepted for publication

Dear Dr. Gigliotti:

There are further minor clarifications in response to your revisions of January 24, 2006.

- Please clarify the relationship, if any, between the 489 women studied in the 2002 article and this study. If the 294 women in this study are among the 489 on whom you reported earlier, you must explain this relationship under procedure. If there is NO overlap, no change is needed.
- If the samples overlap, you must cite the 2002 article under Procedures and Samples and draw the readers' attention to the similarity in the sample here and in the 2002 article.
- On page 12, 1st paragraph, sentence beginning with "Critical N was 60.774..." - please check this sentence.
- On page 13, Model Two paragraph, last sentence - please check this sentence.
- On page 14, 1st complete paragraph, line 6 - change the ".000" to a number other than 0.

Please provide these revisions as soon as possible so that we may move your paper to publication in the near future.

Please send these revisions in an email to my address. Thank you.

Sincerely,

Sherry Handfinger
Editorial Assistant
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Web site: http://www.nursing-research-editor.com
Fax: 919-966-9736

2/6/2006

February 16, 2006

Dear Dr. Doughtery:

Response to reviewer's comments:

I believe I caused some confusion as a result of my using the term "residual" instead of fitted residual. In some texts, unique error terms are called residuals also.

I am attaching the path diagram for model 1. There are no negative variances (covariances). The negative residuals that I refer to are not the unique error terms but the fitted residuals and fitted standardized residuals. A residual is an observed minus a fitted covariance (variance). A standardized residual is a residual divided by its estimated standard error.

In a good model, fitted residuals should fall symmetrically around 0. That is fitted residuals will be low (e.g. -.08 to .08) and the standardized residuals will be -1.11 to 1.11 as an example. In a bad model (like model 1) fitted residuals and fitted standardized residuals will be large (both positive and negative). A large negative fitted residual indicates that the model overestimates the covariance between the variables. A large positive fitted residual indicates that the model underestimates the covariance between the variables.

An excess of residuals on the positive or negative side indicates that the model systematically under or over estimated the covariance between the variables. In the case of model 1, fitted residuals range from -.267 to 0 and fitted standardized residuals range from -6.369 to -4.892.

Therefore, model 1 systematically overestimated the covariance between the variables covariances.

I think the following revision to the sentence on page 12, paragraph 1, will be clearer.

Negative fitted residuals (-.267 to 0) and fitted standardized residuals (-6.369 to -4.892) show systematic overestimation of the variables' covariances (Jöreskog and Sörbom, 1996).

(I had missed the -6.369 and put the next lowest -6.302)

RESPONSE TO POSTING QUERY:
In response to your inquiry concerning posting this article's review on the web, that is fine with me. I have found this process to be quite instructive.

Thank you again for your continued consideration,
Eileen Gigliotti