Reviewer’s report

Title: Understanding clinician attitudes towards implementation of guided self-help cognitive behaviour therapy for those who hear distressing voices: Using factor analysis to test Normalisation Process Theory

Version: 0 Date: 23 May 2017

Reviewer: Cadeyrn Gaskin

Reviewer’s report:

Thank you for the opportunity to comment on the statistics used in this paper. The authors have endeavoured to use Field's textbook to guide their analysis, with software (SPSS) that is not kind to researchers who wish to use the best available strategies for performing exploratory factor analysis (EFA).

Conceptually, I wonder if it would have been better to use confirmatory factor analysis (CFA). The authors' first aim was to test the validity of the four-factor NPT model, which would seem to indicate that it would be useful to see if the data fitted the model (i.e., CFA, not EFA). That is, they appeared to have the hypothesis that the data should have four latent factors. CFA is used for hypothesis testing, not EFA (see Field, p.674).

Given the authors did choose EFA, some comments on their decisions when carrying out this analysis may prove helpful. With the large number of variables per expected factor and the wide communalities, the sample size is probably adequate. Kaiser's greater than one rule and scree plots, however, are known to be inaccurate methods for deciding upon how many factors to rotate. Field touched on this issue in his book (p.679). That is, there may be more factors in the authors' data than can be determined using these outdated methods. For more information, the authors are referred to:


The choice of an oblique rotation, and the actual method, were sensible.

I'm unsure that the analyses using Cronbach's alpha adds anything to the paper. This statistic (although often used) is a rather weak form of validity. EFA and CFA are much more powerful ways of assessing validity.

The inferential statistics would benefit from the reporting of effect sizes in addition to p values.

The authors have interpreted (in the Discussion) the statistically significant difference between qualified therapists and non-therapists on the coherence subscale as something that is potentially
meaningful. It would seem difficult to judge, however, what a 0.42 (2.94-2.52) difference on a 7-point Likert scale means in terms of practice. Perhaps not much.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
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No

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