Author's response to reviews

Title: Prevalence and associated factors in burnout and psychological morbidity among substance misuse professionals

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Version: 3 Date: 6 December 2007

Author's response to reviews: see over
December 6, 2007

Dear Professor Norton,

Re: Prevalence and associated factors in burnout and psychological morbidity among substance misuse professionals

Thank you for considering the above-titled manuscript for publication in BMC Health Services Research. We were pleased that two of the three reviewers were satisfied with our revision.

As requested, we have addressed all comments from the third reviewer and the statistician. We are grateful for the reviewers’ comments and the statistician suggestions. We hope you would now accept the manuscript for publication.

Our point-by-point response to the reviewers’ comments is attached. Thank you for your cooperation.

Yours sincerely

Dr Adenekan Oyefeso
Reader
Point by point response to reviewers’ comments

Reviewer’s report
Title: Prevalence and associated factors in burnout and psychological morbidity among substance misuse professionals
Version: 2 Date: 28 August 2007
Reviewer: Patrick Bovier
Reviewer's report:

General
The authors have responded to my previous comments, however several points are still problematic. I have serious doubts about how the logistic regression analyses have been conducted and how the predictors have been used in these models. The advice from a statistician is very much needed…

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Abstract
Instruments used have to be cited in the methods. The authors should also mention the rates of psychological morbidity and burnout in the results, then cite the main predictors, then mention the different types of stressors. A minimum of quantitative results (e.g. OR and 95% CI) are needed. The conclusion of the abstract should correspond to the main aim of the study (e.g. In this sample of substance misuse professionale, the rate of psychological morbidity and burnout was high, indicating that they are more vulnerable to burnout than other health professionals…)

We have complied fully with all the comments relating to the abstract

Main text – background
Unless specifically permitted by the editors, please avoid bullets in text… I would
propose to transform these parts of the introduction into separate paragraphs, as usually found in the scientific literature (for « Aims and objectives »). The way it is actually presented resembles to a research protocol rather than to a scientific paper…

*We have removed the bullet points and have upheld the reviewer’s suggestions relating the background section*

In addition, I would suggest to include the « study hypotheses » in the method section, to justify the choice of predictors, possibly as a shorter paragraph. The last paragraph under « Study hypotheses » clearly belongs to the methods section… You could also ignore it as this information is repeated in the methods section.

*The hypotheses have been moved to the methods section to increase clarity*

Results
Page 9 : I would move the 5 paragraphs under « Job stressors » of the results section after the paragraph describing the 112 job stressors of the methods section. Your paper is not about this scale and this information does not fit well in the results section. This scale is used to explain how job stressors influence psychiatric morbidity and burnout. You could also reduce its length, as you provided the full list of items in an Appendix.
The name of the third factor « Apprehension about family addiction » is a bit surprising, as these items also reflect fears about their own risk for addiction or HIV infection. Maybe a better term could be «Apprehension about health problems in relation with work »…

*As suggested, the section on job stressors has been moved to the method section, with the factor analysis table moved to Appendix 1*

*We have undertaken a new factor analysis using the procedures suggested by the statistician. This procedure eliminated a fourth factor*
Page 12: The rational for using a multiplicative model should be explained in the methods section, as well as the hypothetical models. In addition, you should use the logit notation for these models... The way you noted these equations is spurious...

*We have addressed these comments. Please see page 13 for the logit notations.*

Page 13: As all results are presented in a table, you do not need to repeat all the results (95% CI) in the text...

*We have addressed this comment (see page 14)*

Tables
I appreciate that you now provide the list of job stressors you have used and the corresponding principal component analysis in Tables 1 & 2. However, I would strongly suggest that you combine these 2 tables into one single Appendix, with a footnote mentioning the second order analysis, and the answer scale to these items (no pressure, slight pressure, ...). I would also appreciate if you could say whether loadings on the other factors were < 0.4. You present only the loadings for the factors you have identified, but some are very close to .4... Do they also load on the other factors? Finally, I would avoid to present the variance and cumulative variance in this table. You can either mention it in a footnote or in the methods section.

*We have conducted new factor analysis using the procedures recommended by the statistical reviewer. These procedures eliminated the need for second order factors. Furthermore, we have included all factor loadings as requested by the reviewer. Items that had a loading of 0.4 or more on two or more factors were excluded – this is recommended practice. (See relevant reference and Appendix for a list of all job stressors)*

Table 3: You need to provide the actual p-value corresponding to the F statistic test. The « n.s. » notation is not sufficient...

*Table 3 is now Table 1 in the revised manuscript. We have provided the actual p-values as requested.*
Table 4: Regarding the logistic regression results, you need to improve your notation: e.g. for « tension », you compare the respondents with a value over the mean to respondents with a value under the mean... You could simply qualify this factor as « High level of tension » in the methods section. (same remark for alienation, e.g. « High levels of alienation »).

*We have addressed this comment in Table 2*

For the age and professional categories, I have a doubt about how you presented your results... In general for this type of categorical predictors, there is always a reference group, for which the OR is 1.0 by definition. I have the impression that you have defined 3 dummy variable for age, one for each category... This is not correct, because you can only belong to one... The same is also true for professional categories, unless some respondents have several roles... Also, it seems that you used ICC and tenure as continuous predictors. This implies that the relationship between these predictors and the outcomes is linear... Is it the case? In general, it is better to categorize continuous predictors in 3 or 4 groups. At this point, the advice from a statistician is probably very much needed...

*We addressed these comments as follows:*

*Age has three dummy variables – under 25, 25-34, 35-44. The referent variable here is 45 year &over with an OR of 1. Similarly, nurses were the referent group for the occupational group dummy variables. ICC and tenure were dichotomised. Participants that scored mean and above were in the high ICC and long tenure groups*

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Page 6, 1st paragraph: « The sampling frame was based on the number of services listed in the directory... »

Last paragraph: « Burnout was defined as a high score... and a low... ». Although you defined burnout, you did not use this outcome as a predictor..., but rather looked for predictors of EE, DP or PA... I would appreciate if you could correct this in the text.
Page 7, last paragraph: « When a factor had more... »
Page 11, last paragraph: « Point-biserial correlation analysis revealed significant positive relationship between PM... »

We have made all necessary corrections

Discretionary Revisions (which the author can choose to ignore)
What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions
Level of interest: An article of importance in its field
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
Reviewer's report
Title: Prevalence and associated factors in burnout and psychological morbidity among substance misuse professionals
Version: 2 Date: 26 November 2007
Reviewer: Michael Höfler

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
1. p.6f.: It is not true that you have only 2 dependent variables because on p. 11 you report results for three different burnout variables.

We have corrected this sentence. There were four variables in all

2. p.9: There is no logical relation between Cronbachs alpha and reliability. Alpha solely measures item consistency. Don't use the term "reliability" here.

We have replaced the term ‘reliability’ with ‘internal consistency’ (see page 9; 11)

3. p.9: With 112 items and 3 factors you are estimating 336 parameters in a sample out of 280 persons. You should better omit items beforehand before running PCA (omit those which decrease alpha, if omitted).

4. p.9: PCA is known to yield more factors than usual (maximum likelihood) explorative factor analysis. Maybe you wouldn't need second order factors then, which are not familiar to most readers.

We adopted the suggestion provided and have reported the results accordingly. The procedures recommended eliminated the need for second order factors (Thank you).

5. p. 13: Interactions in terms of odds ratios are independent of causal synergy (Greenland S. Basic problems in interaction assessment. Environ Health Perspect 1993; 101(Suppl 4): 59–66). Risk differences would be more informative but there is no way to do a better analysis in SPSS.

We have replaced the term ‘influence’ with association and related words.

7. p.13: Don't mis-interprete odds ratios (e.g. "nineteen times more likely"); it would be actually "nineteen times AS likely") as risk ratios.

This point has been addressed (See page 14 paragraph 1)

8. p.17, 1st sentence of "conclusions": Inference should be only drawn on the specific population you investigated.

We have specified the study population accordingly (See page 18, last paragraph)

9. Table 4: The

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
2. p.6: You need to provide 1-2 sentences about possible response bias already here.

We have provided a statement on non-response bias (See page 7, last sentence of the first paragraph). We also addressed this issue under study limitations (page 17)

1. p. 8: How many dummy variables were used to adjust for occupational status?

There were six occupational groups, so we had five dummy variables with nurses as the referent category. This is clearly stated on page 10, paragraph 2

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Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions
Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: I declare that I have no competing interests