Reviewer's report

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

Version: 1 Date: 17 April 2007

Reviewer: Patrick Bovier

Reviewer's report:

General
This is an interesting paper that explored the prevalence of burnout and determinants among substance misuse workers. However, several points still need to be improved before publication.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
Based on the data presented in the paper, the authors need to add the following information:

1. The authors choose to present only the results for high scores on the DP and EE scale, but not for the PA scale. This is not acceptable, because the PA scale is an important aspect of burnout and comparisons can be made with other studies. Alternatively, some authors defined burnout as a high score on either DP or EE scales, but this is not what has been reported here. I suggest that the authors report the descriptive results for the 3 scales, including mean scores and SD, and use the "combined" criteria to identify predictors of burnout.

2. The authors decided to include only predictors that affected at least 66% of the respondents... Why ? I understand that they had to make a choice over the 112 related stressors, but why choose this criteria ? Unfrequent stressors can have an important impact. This point needs to be discussed in the method section.

3. The discussion lacks some comparisons with other studies conducted in similar health professionals groups. Based on table 1, it is also necessary to discuss whether the results of the analyses still hold across the different health professional groups. Because of the heterogeneity of the sample, this point needs to be addressed in the method section. One possibility would be to adjust the results for age, gender and professional groups.

4. Altough the answer rate is excellent after only one reminder, the authors do not discuss how the non response bias could have affected their results. Was the response rate similar across age, gender and professional groups ? If not, how can it affect the results ? This point needs to be adressed in the methods section and in the limitations.

5. I am not in favor of presenting the figure as "A predictive model of burnout...". Previous paper on the same topic have shown that models based on cross-sectional data are not not reliable and are definitely not "predictive" (Mac Mannus). I would even suggest to suppress this figure, or present it "Associations between stressors, components of burnout and psychological morbidity".

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

The abstract length needs to be reduced to comply with the journal requirements.

The introduction should end with the study objectives and hypothesis. The paragraph on the major benefits to measure burnout could be moved after the second paragraph of the Background.

Regarding the tables, the beta coefficients and the constant term can be removed from the tables, the authors should also specify on the tables 2-4 that these are multivariate predictors.

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.