Reviewer's report

Title: Alexithymia, burnout, depression, and family support among Greek nursing staff

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Reviewer: Aino Mattila

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The authors of the manuscript deal with quite an important matter for the nursing profession: how emotional abilities, social support and depression are associated with occupational burnout. There are certain merits to this manuscript. However, there are a few issues I would like to point out as in its current form the manuscript is not suitable for publication.

1. Is the question posed by the authors new and well defined?

First of all, the topic of the present study may be a trifle out of scope of Human Resources for Health as it does not directly deal with “planning, producing and managing human resources for health”. However, it does address the problem of occupational burnout in nursing staff and may therefore be seen as part of this field, too.

A major concern is that the rationale for the study is not very clearly defined. There is no explicit hypothesis for the study in the Background section. There is a sentence in the Conclusions section: “Considering alexithymia as a relatively constant characteristic of personality we tried to interpret its effect in the depressive symptoms and professional burnout.” It would be very advisable to express a clearly defined hypothesis in the Background section. Thereby it would be much easier for the reader to assess the validity of the methods and the significance of the results.

The question of the associations between alexithymia and occupational burnout is rather novel. However, there is a vast literature on the associations between alexithymia and depression (see for example studies by Honkalampi et al.). It has been established in large samples, also with follow-up designs, that alexithymia and depression are significantly associated.

The selection of references is quite narrow. Roughly half of the references are publications by the authors themselves leaving about a dozen other references. The Background and discussion sections are therefore not very well tied to the existing literature.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

One of my main concerns is the operationalization of occupational burnout. The
authors (apparently) use raw sum scores of the three subscales of a Greek version of Maslach Burnout Inventory (MBI) and then report prevalences of those participants exceeding the Greek cut-off points on each of the three subscales (38.9%, 46.3% and 49.5%). Only after this they report the prevalence (14.7%) of those who had ‘pathological scores’ on every subscale. In the subsequent analyses the authors use only the scores of the three subscales. In other words, they do not attempt to evaluate the association between burnout and alexithymia, depression and family support but associations between the three facets of burnout and the other variables. As occupational burnout is usually seen as a syndrome comprising three facets, it may not be appropriate to outstrip the composite MBI scores and use solely the subscales as separate variables. By doing so, the authors, in effect, claim that also those with elevated scores on only one subscale had burnout.

The measures could be more precisely described. Were the five negatively keyed items in TAS-20 converted? There are also positively and negatively keyed items in the MBI. Also, examples of questions on the TAS-20 and Julkunen family support scale could be given as these scales may not be very known to a wide audience.

In the Methods section, two statistical procedures are mentioned (t-test and Pearson correlations). However, in the Results and discussion section there is a paragraph reporting the results of stepwise linear regression. The reader can only guess what kinds of hypotheses and models were tested. What was the aim of the test? Were age and sex controlled for etc? Also, the results presented in the aforementioned paragraph do not convey any significances etc. This should of course be corrected.

There is no mention about the distributions of the continuous variables. In my experience, the distribution of MBI is very skewed and may require non-parametric statistical methods (e.g. Spearman correlations and logistic regression) instead of parametric methods (in this case, Pearson correlations and linear regression).

The statistical software used is not named anywhere.

3. Are the data sound and well controlled?

This is a cross-sectional study with a rather small sample and therefore there are also problems with generalizability. Moreover, in the Abstract the authors claim that their study included 100 nurses. In the Methods section they report that 15 men and 78 women agreed to participate. That actually makes n=93. Then again, in Table 1 there is n=95 for other variables and n=79 for family support. N=95 and n=79 can also be calculated from figures given in Table 2. These discrepancies should be corrected and in, the case of family support, also explained.

Furthermore, the reader cannot tell if the correlation analyses were pairwise (that is, the number of observations not reduced down to the smallest number with all
data available) or if the number of observations actually was 79 for every correlation analysis. The same applies to the linear regression analysis. This should be stated in the paragraph concerning the statistical methods. Moreover, the authors should also consider what the actual number of observations in their study was.

There are no data on work conditions such as shift work or working in superior positions. These may have a great impact on burnout and may also be gender-related. Furthermore, the authors refer to the association between burnout and depression in mental health workers. Was the present study conducted in a psychiatric hospital? It should at least be explained why the work-related factors (with the exception of work experience) were not taken into account – and discuss this as a limitation. (And, by the way, what was the direction of the association between BDI and work experience?)

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

I refer to my responses to questions number 3 and 7.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

I am sorry to say this, but the findings merely seem to be notions on the associations between separate variables (sometimes controlling for a third variable). This may be due to the lack of explicit hypothesis for the study. The findings are discussed quite superficially and the discussion part of the Results and discussion section is very bare. For example, there should be a broader discussion on the findings and their relations to the nursing profession. Moreover, there are no topical references on the ongoing state or trait debate concerning the nature of alexithymia and its associations with state phenomena (in this case, depression and occupational burnout). Alexithymia is primarily seen as a personality trait but some researchers claim that it may also be a secondary state reaction to emotionally overwhelming situations – occupational burnout and depression could be these. This is not discussed at all. As to family support, alexithymia may be associated with the perceived family support, but, on the other hand, alexithymia may also make it difficult to benefit from family support.

The authors state that their results indicate the direct and indirect effects of alexithymia on certain variables. Even though partial correlations may imply mediation it should be remembered that this was a cross-sectional study. The possible theory behind the claims should be carefully presented. If not, the authors should talk about associations, not effects.

6. Do the title and abstract accurately convey what has been found?

Here we deal with the issue of the lacking hypothesis again. The aim of the study, according to the authors, was “…to examine the effect of alexithymia on dimensions of professional burnout, on the perception of family support and on depression in nursing personnel”. As noted earlier, the “effect of alexithymia”
refers to a causal mechanism. In the present study, it was associations between alexithymia and the other variables that were investigated. Due to other problems in the manuscript, it is difficult to tell how accurately the results are conveyed.

7. Is the writing acceptable?

As to the terminology used in this paper, I would like to point out a few words that seem to be highly irregular in the context of alexithymia as well as occupational burnout. As far as I can tell, the words “sentiment” and “sentimental” do not appear in contemporary scientific articles on these topics. The authors state, “Newer studies on alexithymia define it as weakness in the determination and the expression of sentiments…” The most common ways to express these two features of alexithymia are 1) difficulties in identifying feelings and distinguishing between feelings and the bodily sensations accompanying emotional arousal and 2) difficulties in describing feelings to other people (see for example Bagby M and Taylor G (1997): Affect dysregulation and alexithymia. In: Disorders of affect regulation: alexithymia in medical and psychiatric illness. Eds. Taylor GJ, Bagby RM and Parker JDA. Cambridge University Press, Cambridge.)

As to the burnout terminology, I suggest the authors read the excellent review by Maslach, Schaufeli and Leiter: Job burnout (Annu Rev Psychol 2001;52:397–422). Again, “sentimental exhaustion” is not used in this context. The words “emotional exhaustion” that also appear in the manuscript are more appropriate.

In addition to these basic problems concerning the terminology, the abbreviations used in Tables 1, 2, 3 and 4 are incongruent, and in Tables 3 and 4 they are not explained. This makes interpreting the results quite difficult.

The language needs revision as there are other unusual expressions in addition to those mentioned above.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.