Author's response to reviews

Title: Psychosocial Health Risk Factors and Resources of Medical Students and Physicians: a cross sectional study

Authors:

Edgar Voltmer (edgar.voltmer@thh-friedensau.de)
Ulf Kieschke (kieschke@rz.uni-potsdam.de)
David L.B. Schwappach (david.schwappach@isgf.unizh.ch)
Michael Wirsching (mw@psysom.ukl.uni-freiburg.de)
Claudia Spahn (claudia.spahn@klinikum.uni-freiburg.de)

Version: 2 Date: 28 May 2008

Author's response to reviews: see over
The authors wish to thank both referees for their valuable comments and suggestions. The manuscript has been revised along the lines indicated and we feel that the paper has improved considerably. Here is a summary of the changes made.

Reviewer: Barbara Buddeberg-Fischer

Reviewer's report:

ad abstract, first sentence in results
1. It should be clearly stated that this study is a cross-sectional and not a longitudinal one.
   ➔ We now explicitly state that this was a cross-sectional survey study in the methods section and also revised the results section accordingly.

ad background
2. More actual literature of similar studies should be comprised (e.g. McManus, Buddeberg-Fischer et al., Tyssen et al).
   ➔ The requested literature is now included
3. The term "impairment" (WHO definition, means "Schädigung") is too strong and should be avoided in this context.
   ➔ We added the terms "stress" and "psychosocial symptoms". However, since health outcomes in physicians also include substance abuse and suicide we prefer to stick with the term "impairment" which is also in concordance with the international literature (see e.g., Roberts et al. 2005: "Physician impairment is defined by the presence of a physical, mental, or substance-related disorder that interferes with the ability to practice medicine competently and safely", Boisauvin & Levin 2001.) We hope that this is acceptable.

ad methods
4. The description of the sample is missing. In the physicians’ sample their speciality should be mentioned. At the beginning of "results" it is misplaced
   ➔ We now provide the characteristics of the sample in the methods section. As we surveyed students and physicians at an early stage of their career, we did not include questions about medical speciality.
5. In the first sentence of "instruments" AVEM has to be referenced
   ➔ Now included
6. page 6, second paragraph, "Cluster analyses...": The literature of Schaarschmidt should be inserted
   ➔ Now included

ad statistical analysis
7. The statistical analysis should be indicated for each hypothesis with independent and dependent variables (two factorial analyses of variance MANCOVA: independent variables: gender and addressed subsample (first year, fifth year, physicians))
   ➔ As requested we now describe the analysis in detail in the methods section. We also provide new data resulting from two-way analysis of variance as requested.

ad results
8. It is necessary that in all the analyses covariates like age, civil status, specialty have to be included. Thus differences between groups can be considered. This is an essential point in the analyses.
   ➔ As recommended, we now present results of twofactorial analysis of variance with gender as independent variable and the results controlled for marital status as covariate. While we agree on the general relevance of age for the outcome studied, we did not control for age in our analysis due to the fact that age is highly (auto-)correlated with study group (first year students are systematically younger than fifth year students). In addition we restricted the analyses of the physician sample to subjects with 3-8 years of professional experience. Thus, we believe that by adjusting for age, stage of career effects are controlled for, and vice
versa. Due to the high natural correlation between the two, there is no easy methodological way at hand to distangle the effects of age and stage of career since both are.

ad discussion
9. It must be adjusted to the results of the covariance analyses. The meaning of the different response rates in the groups have to be discussed.
   ➔ We now discuss the results in light of the results of the new analyses. We also point to the limitations of the study and in particular to the response rate in the physician sample.

ad table 2
10. Means and SD should be indicated, also main effects of gender and addressed subsample and their interaction.
   ➔ Table 2 has been completely revised.

Reviewer: Patrick Bovier
Reviewer's report:

Major Compulsory Revisions
1. There are three main problems with this paper. First, I strongly suggest that the authors revise their hypothesis, to exclude any possible conclusion that would suggest a temporal trend !!! (cf 1st and 2nd hypothesis…, p. 4) They should simply state that they have studied how health patterns vary across different groups of medical students and young physicians. As the authors acknowledge in the limitations, only a longitudinal study could show such a trend.
   ➔ As suggested we revised the hypothesis. In addition we revised the text and discussion to avoid the impression that this was a longitudinal study.

2. Second, as convenient samples were used in the study, this raises the issue of confounding and selection bias. The comparisons with physicians poses several problems, because a much lower participation rate was achieved (32% vs. 86% and 76% for 1st and 5th year medical students respectively…) and because socio-demographics characteristics were not alike. These differences do not preclude any possible comparisons, but every precautions should be taken to minimize confounding. Results should be at least adjusted for gender, age and marital status. The issue of differential response rates should also be discussed to address this potential source of selection bias. Several statistical methods, such as post-stratification or propensity score to respond can be used to explore how responders differ from non responder (cf ref 21, Goehring et al.) and how these differences can affect the measures and results.
   ➔ We now include detailed sample characteristics and a description of the sampling procedure. We selected cohorts from more than one university and surveyed them in required courses in order to achieve a high response rate. In addition, the physician sample was restricted to a very homogenous group, namely, those with 3-8 years in practice. We would thus not label the study groups as convenient samples. However, these suggestions helped to revise the methods section. We also provide some data regarding differences between responders and the entire sample regarding selection bias and refer to it in the discussion. Regarding the adjustment of sample characteristics, please see also comment to reviewer 1, Nr. 8.

3. Third, I have some problems with the instrument used, the « AVEM »… The authors provide some information in the method section about this instrument, but only one published reference in German. In a rapid search on Google Scholar, I have retrieved some references to this instrument, but all in German… It is therefore difficult for an English speaking audience to judge if the use of this instrument is appropriate. The least the authors can do is to provide a copy of the instrument as an Annex, as well as a translation in English.
   ➔ English references (two articles with descriptions of the instrument by the authors and a study using this instrument) were added and the English version of the instrument is now provided in the Appendix.

Minor Essential Revisions
4. Abstract: It is not clear in the Results section of the abstract whether the data corresponds to three distinct groups ; the results for each group should be clearly mentioned (1st year : 35%, 5th year…, etc…).
   ➔ We now include this information.

5. Page 6 : When the authors refer to Figure 1, it is not clear whether the data that are presented belong to ref. 15 (paper by Schaarschmidt et al, in German) or not… If this is the case, this
6. Page 8 & 9: provide exact p-value (e.g. 0.049 and not <0.05...).
   ➞ We now report exact p-values throughout.

7. Page 4: the authors mention that the survey had been conducted during a required course at the university. Which course? How much time was left to them? How were the participants informed about the study? It seems to me that the students were a little bit « captive » during the survey... Could this have influenced their answers? It surely influenced the answer rate! It is also necessary to mention how the survey presented to the physicians (was the AVEM the only instrument used, was it part of a longer survey, etc...).
   ➞ We now provide details of survey administration in the methods section.

8. Discussion, page 10: The Risk type B is considered by the authors as equivalent to burnout syndrome, and therefore they use it in the discussion to compare the prevalence of burnout in their samples with the published literature. This is not correct!!! This is really comparing « Apples and oranges ». Most published literature about burnout used the three scales of the Maslach Burnout Index as the measure of burnout, and reported prevalence are generally a composite measure of this three scales, or sometimes of only the emotional exhaustion scale. However, I would be very interested to read to what extent these two different instruments are correlated!
   ➞ We now include this information and adapted the discussion accordingly.

9. Table 2: the authors could include also results for each sample (mean), in addition to results by gender; please mention also number in each groups (male (n=...) female (n=...)).
   ➞ Table 2 has been completely revised and now includes the requested data.

10. Figure 1 & 2: the authors should explain what is a « stanine » score it was used (e.g. a method of scaling test scores on a nine-point standard scale with a mean of five and a standard deviation of two) and why...
    ➞ The legend of figure 2 now includes information on the Stanine score.