Reviewer's report

Title: What about teachers’ health? Results from a French survey

Version: 1 Date: 3 January 2006

Reviewer: Pascal Wild

Reviewer’s report:

General

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

1. The population of teachers and controls are not comparable as shown in Table 2. The major problem is the large percentage of subjects with a low educational level (37% no baccalauréat in controls vs. less than 2% among teachers – even this seems high). The education level is associated with mental problems and with several other factors (seniority, life style). More importantly it is likely that the response rate differs according to education levels. This biases the comparison between controls and teachers. I would suggest to remove from the study population all subjects without a baccalauréat.

2. The statistical analysis is unclear and seems at least partially inadequate. In the second paragraph of the methods section, the authors state “In order to take into account the impact of a random selection method, the sample was weighted... All analyses were performed using the weighted population”. It is not clear what the authors mean by weighting the population and why this should be done on “their geographic spread” whatever this means. If the analyses were weighted using weights coming from elsewhere (e.g. from some insurance statistics), this would lead to overweight sparse data, which is neither adequate nor useful for the comparison of teachers and controls. If there is a geographical distortion between teachers and controls, this should be included in a stratified or a multiple analysis. Anyway, given the distortion in education level and to a lesser extent in age, the crude comparison of symptom rates between teachers and controls (as presented in Tables 3, 5 and 6) cannot be interpreted using a simple chi-square or t test and should be replaced by e.g. a Mantel Haenszel chi-square stratified on age and education level or the result of a multiple (logistic or linear) regression model.

3. A description of the occupational characteristics of the two population is missing. The psychological burden of being a teacher is quite different if the teaching occurs in pre-school, primary school, high school or university. Who are the controls? Purely administrative personnel, blue collar maintenance workers, school managers (many of whom are former teachers)…? If indeed a significant proportion of the controls are former teachers, this population should be kept separate. Also, at least in the multiple (multivariate is a commonly used but wrong terminology) analyses presented in Table 3 the differences in symptom rates should be reported by job category, especially as there is no constraint on the number and size of tables in BioMed Central.

4. Given the above-mentioned weaknesses of the statistical analysis and description of the occupational characteristics, the conclusions drawn by the authors may or may not be true. In particular, the lifetime MDE seems to increase with seniority (Table 3), but it is not clear whether this is an effect of years of teaching (see minor comment n°5).

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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)
1. The terminology is not clear. What is a lifetime prevalence of a symptom? Is this a symptom at any time-point in the past which may have disappeared, in this case the symptom is not prevalent or is it a prevalence, i.e. a symptom that the subjects report at the time-point when they fill in the questionnaire? Similarly, what is the definition of one year symptom, at least one year, symptom in the last year?

2. The lifetime prevalence of MDE is about the same than the lifetime prevalence of rhinopharyngitis/laryngitis which includes theoretically a running nose. Does this make sense?

3. Why were rhinopharyngitis and laryngitis regrouped? For the latter one can presume a recall bias as teachers are much more likely to remember laryngitis. This should be discussed, as this is presented as a major result of the survey.

4. All tables should report not only percentages but also raw numbers on which these percentages are based. Also neither raw numbers nor percentages are presented for the “One year” symptoms.

5. In the multiple analyses presented in table 4, I do not think that seniority is the best measure. If one wishes to assess whether number of years of teaching are a risk factor for mental health, this and not seniority (which for the controls could be years in an office) should be tested.

6. The English should be revised with a native speaker.

7. The discussion should include a discussion of the response rates which are good, but may be different according to the teacher/control and also quite likely among subjects who are on sick leave and currently working subjects. The main analyses could be redone excluding subjects on sick leave, in order to be able to judge their influence.

8. The authors mention in the discussion that they consider only physical disorders that can be plausibly associated with their working conditions. They should give details as to why higher rates of rhinopharyngitis and eczema/dermatitis are plausible among teachers. The possibility of false positive associations based on a large number of tests should be acknowledged.

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 whose findings are important to those with closely related research interests

**Quality of written English**: Needs some language corrections before being published

**Statistical review**: Yes

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