Author's response to reviews

Title: Individual and contextual covariates of burnout: a cross-sectional nationwide study of French teachers

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Author's response to reviews: see over
Dear editor:

We thank you for allowing us to submit an amended version of our article entitled “Individual and contextual covariates of burnout: a cross-sectional nationwide study of French teachers”.

We are grateful to Kirsi Ahola for their additional comments. In this new version, we revised the manuscript to fully address the different points underlined, as detailed in the “point by point” response below.

We hope that this new version will be acceptable for publication in BMC Public Health.

Yours sincerely,

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Reviewers report
Title: Individual and contextual covariates of burnout: a cross-sectional nationwide study of French teachers
Version: 4 Date: 13 February 2009
Reviewer: Kirsi Ahola

Reviewer's report:
Re-review of the manuscript "Individual and contextual covariates of burnout: a cross-sectional nationwide study of French teachers" by Vercambre et al. The manuscript has greatly improved during the revision. The authors have revised their paper according to the feedback they have received. However, I find the paper still quite unready. Below are my suggestions to improve this paper. I find them all "major compulsory issues the authors must respond to".

We are thankful for this comment about our work, and agree that the paper can be further improved.

The abstract
1. I wonder if the original sample was representative or might it have been a random sample? The original sample of 20,099 individuals was representative of the MGEN population as it was selected at random (uniform random sampling) from the national health plan records: as explained at the end of the introduction, MGEN health care insurance company covers everyone working or who had worked in France within the public education system or in certain ministries and research institutes. Given the fact that the membership is compulsory, the sampling base was exhaustive. To limit confusing in the abstract, we modified the sentence as follows:
“The source data come from an epidemiological postal survey on physical and mental health conducted in a sample of 20,099 education workers (in activity or retired) selected at random from the health plan records of the national education system”

2. After the sentence "2558 teachers provided complete data", add the percentage (of 3940). We specified the corresponding percentage (64.9%) at the end of the sentence.

3. A sentence regarding the variables used could be added in the methods section.
We added the following sentence at the end of the methods section: “Studied variables referred to demographic characteristics, socio-professional environment, job dissatisfaction, experienced difficulties at work, and teaching motivations”.

4. Instead of terms "last" and "first" quartile, use "highest" and "lowest". We done as advised throughout the text.

5. In the methods- and results-sections, use terms "high emotional exhaustion" and "high depersonalization". We modified the abstract as suggested

6. In my opinion, overall work perceptions is a wrong term to use when the questions inquire after work satisfaction. Throughout the manuscript, we use the expression “work-related factor” instead of “work perception”.

7. In the last sentence of results-section, it is wrong to claim something for influence, since this is a cross-sectional study. Neighbourhood was related to high burnout.

We agree that the formulation was ambiguous. We modified the text in replacing the notion of “influence” by “relationship”/“association” as follows:

“Experienced difficulties with pupils were associated with all three dimensions. A socio-economically underprivileged school neighbourhood was also related to high burnout”.

8. In conclusions, I don’t understand what is meant by teaching background.

We replace “teaching background” by “teaching environment”

**Introduction**

9. In the third paragraph, those individual factors which are reviewed here are not included in the study. However, those individual factors which are included in this study are not introduced. Why?

In the introduction, we present a general review of the literature on burnout covariates in teachers. However, in the data studied, all of them were not systematically documented, in particular, personality traits as underlined in the discussion (“Certain factors that have previously been considered important for burnout were not addressed by the questionnaire used in this study, in particular personality traits and individual resources.”)

To clarify this point from the beginning, we added the following sentence in the introduction: “The choice of individual and contextual variables to evaluate as potential covariates of burnout was inspired by the above literature. To be considered, these should have been sufficiently well documented in the questionnaire used. We also considered some factors that had, to our knowledge, never been studied in relation to burnout, namely the school neighbourhood and the reasons why the teaching profession was chosen.” Please, see also response given to point 11.

10. In the last paragraph, I think you should include the reference of Lee & Ashforth 1996 when proposing to test the hypothesis of the multidimensionality of burnout. They have studied exactly that, even though not among teachers.

We thank the reviewer for this judicious reference. This reference was added in the new version.

11. Add some justification to why you hypothesize that school neighbourhood and teaching motivation would relate to burnout risk.

We developed more precise justifications:

“School neighbourhood is a proxy that evaluates neighbourhood-level psychosocial stressors such as experience of incivility, drug misuse, youngsters frequently hanging around, rubbish on the streets, feeling unsafe, dissatisfaction with the quality of green space, … In the general population, these stressors have been related to level of perceived stress [23], as well as self-reported health [24]. Our hypothesis was that teachers in socio-economically underprivileged school neighbourhood will be more exposed to stressful situations and consequently, more susceptible to burnout.

Regarding teaching motivations, idealism in teachers could be both a protective factor and a risk factor with respect to certain psychological symptoms. Indeed, idealist teachers are probably more psychologically involved at school, because of their concern toward pupil success. Their mental wellness will then depend on their perceptions that reality and ideals go together well or not [25]. We hypothesized that teachers who declared to have chosen teaching because they felt a vocation for that (such as a “desire to make young people grow up”, i.e. “idealistic” teachers) were less susceptible to depersonalization but more susceptible to
emotional exhaustion than those teachers who declared to have chosen teaching for job security.”

Methods
12. Add the percentage after 2558 teachers included (of 3940).
Done

13. In Characteristics of the study sample, first present the fact that "Our sample reflected the proportion of men and women...; then Table 1 presents ...; and thirdly, "In our sample, gender was closely ...". Leave out the The sample description by gender illustrates -sentence and the sentences starting at "Such an association...". I think it is enough that you state that gender was associates with other variables.
We modified the text as suggested.

14. I do not think that reported dissatisfaction with work and experienced difficulties are variables of work perception. Name like "work-related factors".
We replaced throughout the text “work perceptions” by the more generic expression “other work-related factors”.

15. Are there any reliability and validity data on these dichotomous measures? What do they measure and how? Are there any references?
The approach to model a continuous score as a dichotomous outcome on the basis of a specific cut-off point is commonly used in epidemiologic studies. Indeed, this approach, which enables evaluating associations expressed as OR, presents other methodological advantages, in particular, the possibility to distinguish non-linear effects. Here, we choose as cut-off points extreme quartiles of subscores, as done by Brenninkmeyer and van Yperen. These arguments were presented in the “outcomes of interest” subsection.

Results
16. The last sentence in the first paragraph belongs to the discussion. In the results section, say for example that the comparable values in Europe are ...
We deleted the last sentence and replaced it by “Table 3 presents comparable values in Europe, that were picked up from the literature”

17. The last sentence in the second paragraph is not a result; delete it.
Deleted

18. The first sentence in the multivariate analysis -paragraph is not needed; it is said already in the methods.
Deleted

19. The sub headlines are not necessary.
Deleted

20. I think that it is not necessary to highlight the pronounced ORs. I would only refer to Table 5.
We agree and we deleted the corresponding text.
Discussion

21. In the last sentence of the first paragraph, You cannot say that burnout should not be conceptualized as unidimensional. As Brenninkmeyer and van Yperen have written, treating burnout variable depends on the study aims. Say something like for example "...which should be taken into account in study design".

We acknowledge that this sentence was inadequate. We modified it as follows:
“This multi-dimensionality has been previously highlighted in teachers [11] as well as in diverse other professional groups [34], and should not be ignored when designing studies of burnout or intervention in that field”.

22. In the third paragraph, elaborate a little on how gender differences in sources of stress and in social support may explain these observations.

We added the following sentence in the discussion:
“Even when women hold outside employment (as was the case for female teachers in our sample), they still continue to take main responsibility for housework and children [44]. The combined demands of home and work life may result in higher level of psychological strain, whose long-term consequence could be a loss of energy (e.g. symptoms of emotional exhaustion) together with a withdrawal from work (and then a lower level of personal accomplishment).”

We also underlined this important point: “On the whole, gender specificities in burnout were consistent with the sex differences in neuropsychiatric disease vulnerability, with women being at least twice as likely as men to suffer from depression and anxiety disorders [46]”

23. In the fourth paragraph, I don't agree that grade level was a key factor in burnout, rather it was an essential associate, for example.

We replace “key factor” by “essential associate”

24. In the last sentence of the fourth paragraph, I would add "Difference in the work environment and the sources of stress between grade levels..."

Done

25. In the fifth paragraph, I would add as a fourth sentence, "These contextual factors probably represent special additional challenges in teaching."

Done

26. In the sixth paragraph, I do not understand "teaching as a vocation". I think it should be named differently and the idea explained more thoroughly. Also in burnout literature, too much gap between ideals and realism is presented as a risk for burnout (e.g. Schaufeli & Enzmann 1998).

In the new version, we systematically replaced “teaching as a vocation” by “feeling a vocation for teaching”. This notion covers in particular the “desire to make young people grow-up”, as explained in the new version of the introduction.

As suggested, we added a sentence to deal with the case of unbalance between ideal and reality: “However, too much gap between ideals and reality has also been shown to be a risk factor of burnout [49]”

27. In the seventh paragraph, add that there was a two-fold selection bias: first among the policy holders and then among the teachers in active teaching.
We may not have been enough clear in the description of the study design. As explained in point 1., there is not selection bias in the randomly-selected initial sample of 20,099 policyholders. Indeed, the membership to the MGEN is compulsory for all teachers within the national education system. However, there is a selection bias due to non-response as acknowledged in the limitation paragraph:
“[the present study] is vulnerable to selection bias because of its rather low response rate. As a consequence, representativity could not be ensured”

28. Also in the seventh paragraph: I disagree on the statement that there is no reason to suspect that burnout would not influence the accuracy of responses. This holds only to the sociodemographic and other structural factors. It is very likely that burnout influences the perceptions of one’s job and job satisfaction. Bias due to common method variance is very likely.
We agree with your point. We indeed underlined that “Reliance on self-reported data for the measurement of both dependent and independent variables raises concern about the validity of causal conclusions, notably because response could be systematically distorted”. In the new version, we deleted the ambiguous sentence dealing with the link between burnout status and the accuracy of responses. See also response given to following point.

29. One important issue not discussed at all is the reliability and validity of the measures used.
We added a paragraph to discuss this point:
“In our study, outcomes of interest were based on the response to the MBI questionnaire. Other tools are available to measure burnout syndrome but not in French language. The French version of MBI has been previously validated in conditions that were rather similar to the present study (postal auto-questionnaire). In the validation study, indices of internal consistency, long-range stability, factorial validity, convergent validity and hypothetico-deductive validity support the general good psychometric properties of the instrument [28]. Cronbach’s alphas evaluated in our sample of teachers supported further good internal consistency. Concerning covariates of interest, they were constructed on simple ad hoc questions. Response accuracy and reliability was supposed acceptable, but have not been specifically assessed. Again, it needs to be recognized that some associations could have been overestimated since both exposure and outcomes were self-reported. Indeed, individuals with high negative affectivity may perceive their work context more negatively, which would strengthen artificially the associations between burnout symptoms and work environment.”

Conclusions
30. I would move the first sentence to the end as the last sentence.
Done