Author’s response to reviews

Title: The role of collective affective commitment in the relationship between work-family conflict and emotional exhaustion among nurses: A multilevel modeling approach

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Version: 1 Date: 02 Oct 2018

Author’s response to reviews:

Dear Editor,

Thank you for the opportunity to review our manuscript.

We would like to thank the reviewers for their careful reading of the paper and their comments.

We tried to answer all the comments by following the suggested indications.

All our responses are presented below and the changes are indicated in red (track function) within the paper.

Technical Comments

1) Please provide the name of local ethics committee that approved waiver and reference number if possible.

1) We did not have a formal act by the local ethics committee. We reworded the sentence in the manuscript to avoid misunderstanding. Page 7, Lines 179-180 and Page 17.
2) Please move all the figure legends out of the figure files and place them after the References in the main manuscript in a section called "Figures, tables additional files". Note that each figure should be uploaded as a separate file in the submission system (without the legend in the file), and fit on a single A4 page in portrait format.

2) We implemented the request changes.

Reviewer 1’s comments

1) Background:

* Logically, it seems that the last paragraph on page 4 (line 114-122) should belong under the sub-header "Collective affective commitment as a resource".

1) We moved the paragraph under "Collective affective commitment as a resource" section, as suggested (Page 5).

2) Methods

* Data collection (page 6, last paragraph): there is bias to have ward managers and nurse head to distribute the surveys to nurses since the survey contains questions related to work and job burnout - for example, nurses may underreport their emotional exhaustion or overreport their affective commitment. Need to give more detailed explanation who (ward managers and nurse head?) and how (in-person or others?) the written and oral information about the study purpose was provided to nurses. How the survey was delivered and returned?

2) The questionnaires were not administered by nurse managers, but by the researchers. Maybe we used a wrong expression. We reworded the sentence to clarify and better explained the procedure of data collection (Page 7, Lines 184-192).

3) * Data analyses: which software was used to conduct data analyses, Stata or SAS?

3) We used HLM software. Maybe this was not clear in the text, so we better specified in the manuscript (Page 9).
4) * Please explain more clearly how the affective commitment variable is dichotomized into high or low, and how did you choose the cutoff value?
4) We did not dichotomize this variable. As we wrote on Page 11, lines 283-285, we plotted the significant interaction at two levels of collective affective commitment (i.e., +1 SD and −1 SD), and conducted simple slopes tests to examine the nature of the interactions. As showed in Figure 2, the “high” and “low” values are +1sd and -1sd and are automatically calculated by the procedure adopted. We did not use any cut-off value.

5) * Please describe what demographic factors were collected in the study and were they adjusted in the HLM analysis?
5) We added more details about our demographic variables (Page 8, “control variables” section, and in both Pages 11 and 12 of the results). We considered gender and unit tenure as control variables but we omitted results because they were not significant. However, according to the reviewer’s point, we updated these results in Table 2.

6) Some Minor Edits
* On page 12, line 304, please write AC in full name "Affective Commitment" before using the acronym.

6) Thank you. It was an oversight. We wrote the full name for consistency (Page 13).

7) * On page 13, line 331, "consequently worsen the quality of nursing practice" should not be stated here since it is not part of the results of this study.

7) We addressed the inconsistency (Page 14).

Reviewer 2’s comment
1) The article claims to focus on female nurses "for whom work-family conflict is a crucial problem ... as this profession is dominated by women, who have more family responsibilities than many men." Yet the sample consists of only 59% of nurses who are female (no information given on the male nurses - were they from Psychiatric or ER nursing.)
1) Nurses came from different wards of all the four hospitals. The focus of the paper is not on women, nor on revealing differences between genders. This is the reason why we did not give specific information on male nurses (we took the information for granted as we reported the percentage for women, so the remaining part is for male). However, as the sentence generated confusion, we preferred to delete it (Page 2).

Response rate to questionnaires was only 59.5% - why was this? What form of words was given to invite subjects to complete questionnaires? Were there biasing factors here?

1156 questionnaires were administered to cover all the wards of the four hospitals. For some of the wards, the response rate was very low and the data were removed. We saved units with at least 5 nurses per unit. A probable explanation of this loss may be due to the fact that nurses had to complete the questionnaires in one month based on specific indication by the hospital Administrations. This is a brief lapse and it may be a biasing factor explaining the study response rate. Nevertheless, about 60% of response rate is a good rate for organizational health studies, and the sample is appropriate to perform multilevel analyses (647 nurses from 66 units).

Verbal information about study purpose and modality was given to the participants during programmed meetings with nurses and nurse coordinators. Also, a cover letter accompanying the questionnaire explained the objectives of the study and assured participants that responses were confidential. Participants returned their completed in locked boxes (we integrated this information in Page 6, “Procedure” section).

2) No information on reliability and validity of instruments employed is given.

2) We integrate more information on Pages 7 and 8. The reliability coefficients of all the three instruments were already presented in the manuscript (Page 8). However, we performed CFA to test the distinctiveness of the constructs.

3) No information is given on 'back translation' procedures for using culturally imported scales.

3) We added more information in Pages 7 and 8, “Measures” section.

4) No information is given on the crucially important "Affective Components Measure" (reported in Italian by Pierro in 1992, in an unavailable journal). For replication, this scale should be appended in an English translation.
4) Pierro’s scale Italian version is the most used in different contexts by several Italian authors whose studies were published (e.g., Manuti, et al., 2016; Farnese et al., 2018). (please see: (1) Farnese ML, Livi S, Barbieri B, Schalk R. “You Can See How Things Will End by the Way They Begin”: The Contribution of Early Mutual Obligations for the Development of the Psychological Contract. Front Psychol. 2018;9:543. (2) Manuti A, Spinelli C, Giancaspro ML. Organizational Socialization and Psychological Contract: the Vulnerability of Temporary Newcomers. A Case Study from an Italian Call Center. Employ Respons Rights J. 2016;28:225–245).

We did not append the English translation because the scale is not based on nursing context and it wouldn't be suitable for our study. We adapted the scale to the study context.

5) The statistical modelling employed explains less than half of the variance in the outcome measure(s). This could be due to (a) lack of validity of the predictor variables; the paucity of the model, in which too simplistic view of outcomes is made; (3) heterogeneity of subjects.

5) Concerning this point, It is not possible to obtain a true R-squared value in HLM; however, there are statistics that provide a value of the total explainable variance that can be explained by the model, and they are often referred to as R-squared or pseudo R-squared values. We preferred to do not provide any pseudo-R-squared value because of the difficulty to interpret these results.

Finally, our model is not the first “simple” model in the literature. We opted for the principle of parsimony in testing our hypotheses. It would be great for us testing 10 or more variables at the same time. However, it may not respect the scientific approach finding a simple explanation for a complex phenomenon. According to Snijders (2005), adding more variables can create bias in the estimates of the effects of other variables, given the complexity of the error structure and the number of potential cross-level interactions (for example). In this sense, models that include large numbers of fixed and random effects can become unwieldy, difficult to interpret, and perhaps even impossible to estimate.

6) Since the original sample is quite large, I suggest that it is split in two (382 women, 265 men) to see if separate trends emerge. Hypothetically, the model will work best for women.

6) We thank you for this suggestion. Nevertheless, our focus is neither on women nor on testing the differences between genders. Splitting the sample in two would mean to change the focus and the whole paper in both introduction and discussion. The current focus is not to find the differences between male and female, but is testing a WFC multilevel model for nurse profession as the nature of this work (e.g., shift work, long working hours...), thus suggesting
managerial strategies to mitigate WFC and reduce emotional exhaustion independently from gender. However, we treasured this suggestion for future research with different aims.

7) ADDITIONAL REQUESTS/SUGGESTIONS:

Please see comments above on validity of instruments, low response rate, and high proportion of male nurses.

7) We tried to answer all the requests in the above sections.