Author’s response to reviews

Title: COPING BETTER WITH HEALTH PROBLEMS AFTER A VISIT TO THE FAMILY PHYSICIAN: ASSOCIATIONS WITH PATIENTS AND PHYSICIANS CHARACTERISTICS

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Version: 1 Date: 04 Jun 2017

Author’s response to reviews:

Dear Editor,

Thank you very much for your comments and theses constructive reviews. Please find my responses to the reviewers and a new version of the manuscript, taking into account the reviewers’ comments.

In this revised manuscript, the corrections and changes appear using track changes (except for the tables).

I hope this new version of our paper will be satisfactory to be published in BMC Family Practice. Kind regards,

Dr Christine Cohidon

Reviewer 1

Thank you very much for your positive comments and your interesting suggestions. Please find below my answers to the reviewer comments and in italic the element added in the revised manuscript. The changes in the revised manuscript appear using track changes (except for the tables).

1. Selection bias

While it was the intent to draw a representative sample of Swiss FPs, the low response rate of 10% among FPs might introduce selection bias as the authors acknowledge for in their limitation section. However, the exclusion of patients answering 'I don't know' in the outcome measure (about 230 of about 1800 patients) is an additional risk to introduce bias. The authors state here in their discussion section 'we also performed the statistical analysis including the 'don't know'
answers and the results were very close’. In summary, I recommend two things here: better description of the sensitivity analysis (how were the ‘don’t know answer’ included) and better organization of the manuscript (sensitivity analysis has to be noted in the methods section and the results in the results section (see also my point 7).

The sensitivity analysis is now introduced and better described in the methods section as the following: “The initial patients sample consisted in 1791 patients; for the present analysis, we excluded those who answered “I don’t know” to the question about enablement. However, we also performed a sensitivity analysis regrouping the “don’t know” with the “no” answers (versus “yes”)”. Moreover, the result of this analysis is now mentioned in the results section: “The sensitivity analysis regrouping people who answered “I don’t know” with people having answered “no” to the question of coping better after a medical visit reported close results (not shown)

2. Appropriateness of the statistical analysis

a. Each FP randomly selected about 9 patients. The authors account for the ‘nested nature of the observations’ in the methods section. I would expect the use of a mixed logistic regression analysis accounting for the clustering effect within each FP but I don’t see how the authors did this.

We did perform a mixed logistic regression model indeed and that was mentioned in the methods section as “multilevel logistic regression model” (p8 L113). But for a better understanding, both designations are now stated in the methods section. Moreover, in link with this design, an explanation of the term “practice variance” reported in the table is now provided: “The variance explained at the practice level is reported at the end of the multivariate analysis table as “Practice variance”. Finally, the amount of the practice variance is now also reported in the results section as follows “Finally, the variance explained at the practice level was small (0.04), meaning that most of the determinants of patient enablement are patient-based”.

b. Only about 120 patients reported no better coping after consultation which would allow to adjust for about 12 covariates to be included into the multivariate model to prevent data sparsity. The final model (Table 3) includes 13 covariates. I don’t believe this will already result in sparse data but since this analysis was based on a backwards elimination, I would suggest to adjust for 12 covariates and/or better justify the 13 covariates. Also, I lack a clear description, how the backwards elimination was done. How were the models compared when removing covariates.

I agree with the reviewer that it would have been more conventional to include no more than 12 covariates in the final model. Actually, we had chosen to force some variables in the final model, not significant at 5%, either because they were borderline significant, e.g. effort-reward imbalance, or because they are usually considered as essential variables, e.g., patient gender. Insofar as these non significant variables do not notably modify the other risks, we preferred to keep them in the final model to give a maximum of information. The fact is that, in my opinion, several very close final models could be considered as reasonably valid in this case. However, in
interest of clarity and simplification, I propose to remove from the final model the variables that are not significant at 5%. Thus, a new final model is proposed.

Furthermore, in order to obtain the final multivariate model, we performed a manual backward procedure (the automatic stepwise function is not available using a multilevel analysis (melogit)), eliminating the least significant variable at each step. This precision has been added in the methods section (p8 L123).

c. Did you test for collinearity and/or interaction in the final model? I read 'we also tested a potential interaction between the number of visits and an existing chronic disease but it was not the case' (discussion section). I recommend a clear description why you tested for interaction (a priori chosen or along the analysis and for which other covariates you tested for interaction). I would be interested to know if the main reason for the visit (ill or didn't feel well vs. other reasons) modified the effect.

The collinearity of the covariates was assessed looking at the stability of the models during the manual backward procedure toward a final model.

Moreover we just tested a few predefined interactions along the analysis (number of visit * chronic disease / Other paramedical discipline * language area / FP gender * Other paramedical discipline) in order to better identify the potential origin of the observed associations and to introduce some elements in the discussion. As none of them were significant I propose to remove that element from the paper.

I am not sure to understand your last comment about the reason of the visit. This variable was not associated to the outcome in univariate analysis so we had no reason to look for an interaction. Moreover, you did not precise which association this variable could modify (between enablement and which covariate?).

3. Outcome

I cannot follow the choices around the outcome. While most of the manuscript is around better coping, the outcome for the analysis is reported as 'no enablement' = not coping well with health problems. This switch from positive to negative also happens in covariates (e.g. 'would you recommend this doctor' are reported as 'no') but in the discussion the negative covariates and the negative outcome are then reported as 'enablement was higher when the patients recommend their doctor (OR=4.76 [1.98-11.43])'. The numbers are still right but for the reader it is difficult to switch perspectives along the sections of the manuscript. My suggestion is to stick to one outcome (enablement yes vs. no) and report covariates (e.g. recommend this doctor) as yes vs. no, too.

I totally agree with the reviewer that it could be difficult for the reader to follow if we switch along the paper between negative and positive forms regarding both the outcome and the covariates. For the outcome variable we chose to use the negative form for statistical reasons. The prevalence of coping better with health problems was very high (90%) and thus the Odds
ratios estimated by the models could not be interpreted as a relative risk. As it is a very common habit (to interpret OR as RR), we preferred modeling the negative form. In this context, I also tried to avoid too much awkward sentences for instance using double negative forms!

But, in this revised version, I paid attention to be constant in the form used for the outcome and the covariates. Moreover, for each covariate coded Yes/No, the value “No” is now taken as reference.

4. Effort reward imbalance exposure

The authors conclude that ‘only exposure to job stress, through effort-reward imbalance, among physicians is associated with a better enablement among patients’. However, the results in table 3 show a non-significant association (adjusted OR 0.66, 95%CI 0.42, 1.03). Thanks to clarify if there is an error in the table or the association is not significant and the results/discussion needs rephrasing.

Yes, I agree that the variable was not significant indeed (at 5%, no error in the table 3). However it was borderline significant. And as I explained in 2b several very close final models could be considered as reasonably valid in this case. In the new final model, this variable is even more close to the significance (OR: 0.64 [0.41-1.00]); however I tempered my statement, introducing the term “borderline”.

5. Measurements

Many covariates have been grouped e.g. in two quantiles according to their median. This only comes clear in the discussion section for e.g. age or in the tables. I suggest you clearly describe in the methods section the grouping and justification of grouping the covariates to help the reader to understand. Also, the measurement of Effort reward imbalance could be better explained in the methods section (Yes vs. no or Likert-scale?).

We introduced some additional details to describe some variables statement in the methods section.

Moreover the ERI variable is now better described saying “Exposure to work-related stress was explored through a proxy of the Siegrist’s effort/reward imbalance model (ERI) assessed using a single item, the following affirmation: “In my work there is a good balance between effort and reward” (strongly agree / agree/ disagree / strongly disagree, then regrouped in “Exposure to ERI yes=agree + strongly agree /no=disagree + strongly disagree” for the statistical analysis”.

6. Residual confounding

In such studies, there could always be residual confounding but when looking for predictors of patient factors to coping with health problems I would be interested to know, what has just happened in the consultation after which the questionnaire was distributed. You have a variable there but maybe stratify according to the reasons for encounter? Another interesting covariate would be for how long the patient is seeing this physician. Is there a proxy you could adjust for
in the QUALICOPC dataset? This would also help you to better argue your claim in the discussion section that 'there might be an ideal number of visits for a better enablement' (line 169-170) because another explanation could be that those not satisfied did not re-consult with the same FP.

There might indeed be some residual confounding, but the relatively small number of negative outcomes for negative coping precludes to systematically explore this effect.

I don’t understand what the reviewer means exactly by “what has just happened in the consultation after which the questionnaire was distributed. You have a variable there but maybe stratify according to the reasons for encounter”. This latter variable was not associated with the outcome OR=1.0 IC [0.67-1.47]. General good statistical practices recommend not testing for interactions in absence of a main effect. However, to be sure, I checked that there was no interaction with the reason of the visit.

We have no additional information to characterize the quality of the patient-physician relationship except the three variables already included in the analyses: the doctor seen is the patient’ own doctor and the number of the visits the last 6 month, and that the patient would recommend this doctor. The latter variable can be considered to assess satisfaction with the FP. In the final model the association between enablement and number of visits exist even after adjusting on these 3 variables. But we added a sentence to say “we can also state that we found a lower ability to cope with health troubles among patients with low frequency of encounters because they do not reconsult with the same FP.”

7. Organisation of the mansuscript

I acknowledge there are different approaches to organize a manuscript but in this case, the paper is well written and clear until about the middle of the methods section and then the information are a bit misplaced. I suggest to move around the following parts and bits of information to the respective sections:

Introduction: Information about QUALICOPC move to the methods section. Results: 'we excluded those who answered 'I don't know' (…)’ move to the methods section.

Discussion: 'Lastly, including or not this variable did not change the results about the other variables in the final model' move to the results section.

Discussion: '(…) the consultation's length was a physician's variable (…)’ move to the methods section.

Discussion: '(…) the low participation rate (about 10%)' move to the methods section. We considered all your suggestions and proceeded to the appropriate changes.

Minor points
All the minor points have been taken into consideration and the appropriate corrections have been made.

Reviewer 2

Thank you very much for your positive comments and your interesting suggestions.

Abstract

Methods - you report a random sample of 200 physicians and 1751 patients that participated in the study; in the Methods and Results section of the manuscript you report a sample of 199 physicians (line 66, 119) and 1791 patients (line 70, 115) - please adjust

The number of physicians and patients has been corrected in the abstract.

1. Introduction

Lines 49 - 50 - "to our knowledge, the relationship between family physician work stress and patient coping has never been investigated" - I would suggest the reference on a similar topic: Is Burnout in Family Physicians in Croatia Related to Interpersonal Quality of Care?. Archives of Industrial Hygiene and Toxicology, 64(2), pp. 255-264.

Thank you so much for this reference that I missed! I have introduced it at the end of the introduction section (p4, L52).

2. Methods

Lines 85-86: please provide more details regarding the variable "number of visits to a FP in the last six months" - was it self-reported by the patient, or extracted from patient's medical record? This variable is now précised (p7 L91)

Lines 93-94 - provide detailed information regarding variables stemming from ERI model the ERI variable is now better described in the methods section saying “Exposure to work-related stress was explored through a proxy of the Siegrist’s effort/reward imbalance model (ERI) assessed using a single item, the following affirmation: “In my work there is a good balance between effort and reward” (strongly agree / agree/ disagree / strongly disagree, then gathered in “agree/disagree” for the analysis)”.

Line 101 - "The feeling of coping better with health.." problems?

This has been corrected (p8 L112)

3. Results
Tables 2 and 3 - please state the ref.value for each of the tested variables in the regression models; again, the effort reward imbalance exposure variable is very unclear.

The reference values are now presented for each covariates in table 2 and 3.

Line 134-135 - you report the association between exposure to effort-reward imbalance and enablement in the final model (model 3) - OR 0.66; is it significant, considering CI 0.42-1.03? Please check.

Yes, I agree that the variable was not significant indeed (at 5%, no error in the table 3). However, it was borderline significant. Moreover, when the variable “other disciplines in the practice” (which was not significant either) was removed from the final model, ERI became significant (CI upper limit: 0.99 !). Actually, in my opinion, several very close final models could be considered as reasonably valid in this case. Initially, we had chosen to force some variables in the final model, not significant at 5%, either because they were borderline significant, e.g. effort-reward imbalance, or because they are usually considered as essential variables, e.g., patient gender. Insofar as these non significant variables do not notably modify the other risks, we had preferred keeping them in the final model to give a maximum of information. The fact is that, in my opinion, several very close final models could be considered as equally valid in this case – their Akaike Information Criterion being very similar (data not shown). However, in interest of clarity and simplification, I propose to remove from the final model the variables that are not significant at 5%. Thus, a new final model is proposed.

In this new final model, this variable is now on the brink of statistical significance (OR: 0.64 [0.41-1.00]); however I tempered my statement, introducing the term “borderline”.

4. Discussion

Lines 164 - 165 - the hypothesis regarding older age / longer relationship between the patient and his physician / and higher patient enablement can be underpinned by well-known positive association between continuity of care and patient enablement (ref Howie; Mercer; Ozvacic Adzic).

Thank you for this comment. I introduced the concept of continuity of care as you mentioned (with two references Lam 2014 and Ozvacic Adzic 2013): “These results can be underpinned by the well-known positive association between continuity of care and patient enablement, even though we could not find any association with the consultation duration (see limitations section).”

Lines 169- 173: the association between enablement and frequency of visits needs to be discussed in terms of patient's health status - more frequent visits may indicate poorer general health

I agree with the reviewer that the patient health status has to be studied in the relationship between enablement and frequency of visits. In the final model, adjusting both on longstanding disease and perceived health allow taking into account the patient health. But the relationship
remains significant. This part has been rewritten as follows: “Furthermore a high number of visits, that are the case of patients with chronic diseases (and, we can imagine, leading to higher continuity of care) is not associated with less difficulties for the patient to cope better with health problem. Indeed the association between poor enablement and longstanding illness or poor perceived health has been founded in several studies [7, 14] as we also observed in our data. And finally note also that the relationship between the patient enablement and number of visits persists after adjusting on existing chronic disease and perceived health”. I hope this part is now clearer.

5. Conclusion

Line 245 - "his/her own characteristics" - patient characteristics?

The word “patient” has been added.