Author's response to reviews

Title: Association of self-rated health with multimorbidity, chronic disease and psychosocial factors in a large middle-aged and older cohort from General Practice: A cross-sectional study

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Author's response to reviews: see over
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Dear Editors,

Re: “Association of self-rated health with multimorbidity, chronic disease and psychosocial factors in a large middle-aged and older cohort from General Practice: A cross-sectional study”

Ref number:

Please find below our responses to the reviewers comments:

**Reviewer 1**

I have the following minor comments, which the authors can choose to address:

1. Abstract conclusion: I think could be more focused on interpretation of results in terms of potential to target interventions within the MM population as mentioned by the authors in the discussion

A sentence has been added to the abstract to address this point. (p.3 line 133-)

2. Results:

There were no comments listed here.

3. The age actually goes up to 79 (or 74 also stated in text as upper age limit) so I think it is a little misleading to refer to it as a middle-aged cohort – perhaps middle aged and elderly?

The correct upper age range is 79. We have changed the title to:

“Association of self-rated health with multimorbidity, chronic disease and psychosocial factors in a large middle-aged and older cohort from General Practice: A cross-sectional study”

4. The analysis does take socioeconomic factors into consideration which should perhaps be added as a limitation

Social class and education were collected for all participants the EPIC-Norfolk study. We include one of these measures (social class) in the analysis in the regression model, but education was omitted in order to avoid overcorrection. Previous work in the EPIC-Norfolk cohort confirms a strong association between SRH and social class.[1] We have also now included results of the regression model on the
5. How comparable is the EPIC-Norfolk cohort to the wider population—could be addressed by looking at reported anti-depressant use compared to national figures or SF36 scores compared to national norms. A line has been added describing the comparability of the EPIC-Norfolk to other populations based on functional and psychosocial characteristics to the methods as well as strengths and limitations sections of the paper (p. 7, line 207-; p. 13 line 354-).

6. The authors describe the inclusion of the six conditions as a strength but it is quite limited in number and likely explains how the prevalence seems lower than in other GP based cohort studies, might be good to compare prevalence MM with other studies as well.

Thank you for this suggestion. We now refer to our recently published systematic review (Violan et al. 2014) with relation to the low prevalence of multimorbidity in our study as compared to other studies in the strengths and limitations section of the paper (p. 13, line 362-).

7. Implications of findings: lines 369-372: it seems to be stretching the point to claim that eliciting SRH status may help to predict mortality based on the cross sectional nature of this data and no other reference to support this link.

References 22-24 of the paper do in fact address links between SRH and mortality and other health outcomes. We have now added a further reference from our own systematic review confirming a longitudinal relationship between SRH and cardiovascular mortality. [2]

8. Occasional inconsistencies in use of hypen in MM – see line 259

This has been corrected throughout the manuscript to ensure consistency.

Reviewer 2

Major Compulsory Revisions

1) As the authors themselves state this is an area which has previously been fairly well studied. They aim to add to this research by assessing "whether specific disease combinations may work synergistically in their impact on patients’ health experiences". However, I don't think they really do this in the manuscript. To my mind, this would involve looking at how specific combinations of diseases (i.e diabetes/cardiovascular compare to cancer/cardiovascular only etc) affect self rated health rather then looking at individual conditions and how
increases in the number of non-specified conditions impact.

Our aim in this analysis was to determine whether psychosocial factors mediated the association of SRH with multimorbidity and with physical chronic disease combinations. However, to assess each possible disease combination would have resulted in a large number of separate analysis. We sought a different approach to the problem by clustering the conditions into an ‘index’ condition with the addition of other comorbidities. We feel this novel approach has clinical utility and is of interest to the readership. Nevertheless this analysis was an extension of the primary work to explore the relationship between SRH and multimorbidity with psychosocial factors. If the reviewer feels that this analysis does not contribute significantly to the aims of paper, we would be prepared to remove this table and corresponding figure.

2) For logistic modelling, the authors classify “poor” as those with poor or moderate self health. Given the overwhelming majority of this group are those classing themselves as moderate this presents a somewhat misleading picture. Therefore, I think the authors so either compare by three groups (good, moderate, poor) or relabel the poor group as moderate/poor and give greater emphasis to this in the discussion and conclusions.

Thank you for your suggestion. We have relabelled the “poor” group as “moderate/poor” and the good group as “good/excellent” in the methods and results sections of the paper and the tables. We have addressed this point in the abstract and discussion, by referring to the “moderate/poor” group as those with “poorer” health rather than “poor” health.

3) All the results show differences by gender so the regression modelling in table 5 should do this as well rather than just adjusting for this.

Thank you for this. We have now re-drawn table 5 to show differences in gender. This now highlights even more strongly the differences between genders in the relationship of self-rated and multimorbidity. A few sentences have been added to the results section of the paper to highlight this point. (p. 10 line 294, p.11 line 302)

4) Previous studies have show the importance of mental health in multimorbidity and this is likely to have a bit impact on self rates health given its coexistence with pain (Barnett k et al, Lancet. 2012 Jul 7;380(9836):37-y of approaching this problem without 43-McLean G et al BJGP July 1, 2014; 64 (624). Therefore, adding depression in some form as a condition in the regression analysis would add to the study in assessing the effect of depression on self rated health with increasing multimorbidity.

Since the focus of our research was on the impact of physical chronic conditions on self-reported health status with mental health as a potential mediator, self-reported depression was not used in the count of conditions but as a covariate along with
other measures of psychological and social functioning. A few lines clarifying this has been added to the paper methods section (p. 9 line 25-2). The model in Table 4 already adjusts for self-reported depression. We have also repeated the analysis adjusting for self-reported depression in the model used for Table 5. However, no significant change was seen in the results after adjusting for self-reported depression and therefore we have not altered the table. We feel in our study anti-depressant use to be the best proxy available for current depression.

We have now also referred to the above two important papers regarding the relationship of multimorbidity with depression and socioeconomic factors to our introduction. (p. 6 line 181-).

5) Deprivation has also how to be a significant factor in MM and given the high rates of pain found in the multimorbid in more deprived areas (McLean G et al 2014) likely to have an effect on self rated health. Some additional analysis on this would add greatly to the paper at the very least should highlight the potential effect of deprivation on self rated health in greater detail in the discussion.

Our analyses have already taken social class into consideration. Please see response to reviewer 1 question 4 on this point. We have added discussion regarding SRH and socioeconomic factors in the introduction and strengths and limitations sections (p. 6 line 181-; p. 14 line 380-).

Minor Essential Revisions
1) The authors should report how representative of the population the cohort is.

Please see response to reviewer 1 question 5.

2) Self rated health is listed as moderate in Table 1 but fair in Tables 2 and 3 should be one or other

Thank you for pointing this out. This has been addressed. See tables 2 and 3.

3) Increasing the number of conditions reported would help to give a better idea of increase in poor rated and multimorbidity (0, 1, 2, 3, 4, 5 or more).

We note that as per Table 1, only 0.8% of our sample has three or more conditions, leaving only 19 people (0.08%) with four conditions and with only 1 person with five conditions, so this would not be feasible.

We believe we have addressed adequately the reviewers concerns, which have no doubt resulted in a stronger manuscript that we hope will now meet the requirements for publication.

Yours sincerely,

Nahal Mavaddat