Author's response to reviews

Title: Lack of adherence to hypertension treatment guidelines among GP's in southern Sweden - a case report based survey

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

We gratefully acknowledge the criticism and opinions of the referees and yourself and we have earnestly made an extensive revision of our manuscript which we hope will suffice for publication for BMC.

The following points were addressed:

Regarding patient no 2 it seems that the referee mistakenly asked for a clarification as this patient did not have diabetes.

Further regarding the comments from referee no 1:

1. *The third key point has to be further explained. How was this shown in this manuscript?*

   This has now been omitted.

2. *In the discussion the authors state that selection of the centres was random yet in the methods they give the impression that selection was purposive. Of these 90 primary care centres, 24 centres (employing a total of 109 GPs) were selected with for participation in the survey. The selection was made according to demographic and geographic structure and is believed to be a cross-section of the GP’s of the area.*

   *This needs to be clarified but the authors also need provide the evidence for their belief that they were a cross-section of the area.*

   The GPs were randomly selected from all publicly employed GPs in the Region of Skåne. The reasons for choosing these GPs is that they constitute the majority of GPs in Skåne and that there pharmacotherapy of hypertension has previously been well described (Hedblad et al 2006). It is now described Methods (page 5 line 3-5).

3. *Potential intra-class correlations should be explored. Were there any*
systematic differences between the 24 health care centers regarding GP’s attitudes and considerations? A finding of a practice variation would be of a particular interest for the implications of this study.

We understand this as satisfactorily settled (While I too would be interested in a multilevel analysis, their approach is acceptable so I can accept their decision not to address this point.)

4. The five cases are chosen to represent different patient categories of interest for the study aim. They are, however, not representative for a population of patients with hypertension in primary care. A patient-based study would probably give a different pattern. This should be stated more clearly.

This has been addressed in the final part under questionnaires

5. Implications of the study see below The discussion see below.

This is made more clear in Discussion p 15.

The discussion is split in many short paragraphs giving an unfocused impression.

The disposition has been altogether changed.

Our replies to the comments from Referee 2:

1. The English language needs professional revision.

As mentioned under 4 below such has taken place professionally by natural-born English speaking person.

2. Major: No statistical analyses have been carried out by the authors why the results are purely descriptive, and therefore it is hard to evaluate to what extent there might exist over- or undertreatment, based on judgement on written case histories. However, based on a similar design with clinical case studies, it has previously been possible to apply statistical analyses of results - see reference: "Backlund L, Danielsson B, Bring J, Strender LE. Factors influencing GPs’ decisions on the treatment of hypercholesterolaemic patients. Scand J PrimHealth Care. 2000 Jun;18(2):87-93". If the authors do not want to apply statistics at all they have to provide good arguments not to do so.

This has been addressed in the text, accepted by the referee.

3) Minor: (a) In Results for Case 1 there is a sentence that was duplicated on which BP target was set. A BMI of 25 kg/m2 is not obesity (per definition) as mentioned in Results of Case 1. (b) In Risk Assessment for Case 2 there is mentioning of NIDDM, a term that is not in use any longer.
The correct term is "type 2 diabetes". (c) Diabetes should also be mentioned among major cardiovascular risk factors in the Introduction (first page).

See clarification sought above.

These issues has been corrected: (a) omitted (b) the term is changed © Diabetes is mentioned in p4 line 4.

4) The manuscript needs language revision as there are some mispellings and grammatical errors.

Language: there are remaining grammatical errors
a. P6 line 2 `you¿ vs `your¿
b. P6 4th last line `takes¿ vs `take¿
c. P6 3rd last line `covers¿ vs `covers¿
d. P13 line 7 `Case based questionnaire studies aim¿ vs `Case based questionnaire aim¿
e. P14 8th last line ` publication¿ vs `publication¿

The language in the article has been reviewed for correctness previous to re-submitting. It has now been re-checked and we believe there are no errors in the text provided.

Discussion: I agree with the 1st referee¿s comments and had in my original decision observed that the discussion was `Disjointed and unfocused¿ and recommended that `Using a structured format may help ¿ what we found, strengths and weaknesses, comparison to other literature, what it means, what next - would help its accessibility. More critical examination of the methods/results is required.¿

Extensive revision of the discussion is still required.

The discussion has been extended and also the disposition has been remade I accordance with the view of the referee.

I still lack a comment on possible practice variation, and I still do not think that physicians adherence to guidelines was assessed, but merely their opinions. It does give interesting information on a current clinical topic.

The randomisation taking place has been explained in the text and this paper follows our previous publications Hedblad et al 2006 and Midlöv et al. 2008 which show failure to reach BP targets and possible barriers to the adherence whereas the present paper aims to introduce a clinical setting with other factors added as well.

The manuscript has improved but there are still no tables showing some summarized data. This is a bit strange. I also mentioned about the use of "NIDDM" for type 2 diabetes in case 2, a term that is not used any longer. Now it seems that the authors just deleted this characteristic. This would change the risk categorization.

I think that the authors should be told to cope with my critical comments in an appropriate way and also to reply to my comments and suggestions in a detailed way, not only by mentioning that it is "mentioned in the manuscript".
A table has been added and the second part of the question has been addressed on the top of the letter. We have endeavored to be carefully detailed in our replies to the opinions on the part of the referee.

Hoping these changes will suffice to qualify for publication in BMC, we remain,

Yours sincerely,

Rickard Ekesbo