Author's response to reviews

Title: Profile and predictor of health-related quality of life among hypertensive patients

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Author's response to reviews: see over
Response to Reviewer’s report

Title: Profile and predictor of health-related quality of life among hypertensive patient

Version: 2 Date: 24 February 2009

Reviewer: Christos Lionis

Reviewer’s report:

Dear Editor,

Thank you for inviting me to review this paper from Nigeria that reports issues on quality of life among people with hypertension. Although this paper addresses an important subject for general practice and primary care, it is written in a very descriptive manner and presents certain methodological flaws that make it not acceptable for publication under this format.

Response: These flaws have been attended to.

The introduction is too extensive, without any reference to the conceptual framework on which this study was built, nor the research questions posed by the researchers. Did the authors expect that quality of life of Nigerian patients with hypertension differed from that of patients in regions? Did they make any assumptions that a non-western life-style had a particular impact on hypertension outcomes? Such introduction may increase the interest of the journal’s wider readership.

Response: The introduction has been trimmed down with specific reference made to the conceptual framework proposed by Wilson & Cleary on which the study was built. Also the research question explored in this study has been clearly stated with the main assumption been the possible difference between the HRQOL of black and non-black hypertensives.

There are also some flaws in terms of the sampling methods. The phrase “a total sampling method was used” does not provide sufficient information to the reader. The authors are also invited to provide some information on the power and sample size estimation of the study.

Response: The total sampling method has been explained in the context of this study with the information on the power and sample size provided.

Information on validation and cultural adaptation of both questionnaires used in the study is lacking. This is important in establishing that the concepts of quality of life were understood in the same manner and degree as people from other regions, outside of Nigeria.

Response: Information on the validation of the SF 36 instrument used as documented by Ware et al has been provided. This was assumed valid for the study population. Attempt at cultural adaptation was done prior to the onset of the study by translating the questionnaire to the most common language in the research location. Forward and backward translation was done to ensure internal consistency is maintained.

The discussion section is very long and it contains many repetitions. The authors
should be reminded that in this section they should discuss the main findings, interpret them, and consider potential biases, not simply repeat the findings. I strongly recommend that the authors re-write this section and that they use headings to highlight different parts of the discussion. Limitations of the study are not discussed and the authors are kindly invited to consider a separate paragraph at the end of the discussion section. I am also suggesting a separate paragraph, at the end of the discussion section, presenting the implications of the study.

Response: All the suggested correction has been effected in the discussion.

Level of interest: An article of importance in its field
Quality of written English: Needs some language corrections before being published
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I declare that I have no competing interests.

The authors believe that all the issues raised have been addressed and wish to thank the reviewer for doing a splendid job.

Mr. Ogunlana M.O
Principal Investigator.

Response to Reviewer's report 2
Title: Profile and predictor of health-related quality of life among hypertensive patient
Version: 2 Date: 19 February 2009
Reviewer: Per Wändell
Reviewer's report:
General
This is an interesting as most assessments of quality of life are performed in developed countries, and articles like the present one are welcomed.
Remarks:
Major:
1. Introduction: This part is rather long, the part of HRQOL could be shortened. Assessments of HRQOL are performed in many different conditions in many different parts of the world, and I cannot find that there is any controversy about the importance of HRQOL. The authors could start with a remark that hypertension is considered the leading risk factor for global mortality by the WHO
(ref e.g., Kearney PM et al, Lancet 2005;365:217-223). As regards the impact of hypertension on HRQOL the authors could mention that hypertensive subjects in general showing the least overall impact compared to other chronic conditions, in contrast to subjects with heart failure or myocardial infarction, or depression (ref Wells KB et al, JAMA 1989;262:914-9). The authors could also mention that SF-36 is the commonly used HRQOL questionnaire in the world, with translations into many different languages.

Response: The introduction has been edited and appropriate inputs have been added to drive home the aims of the study.

2. Methods: the authors report the subjects responding, but how many did not respond, and which were the reasons for this. Variables used should be defined.

Response: The subjects that did not respond have been reported and the variables defined as they were used.

3. Statistics: the authors use multiple linear regression, but it takes some time to interpret the results. As I understand presence of obesity of visual impairment, respectively, are reference instead of the opposite; this should be changed in order to facilitate for the readers. Besides, some variables are non-linear, which causes problems, e.g. symptom count, educations status. This could be solved in different ways, an easy way is to dichotomize data, i.e. in Table 9 any education (primary+secondary+tertiary) vs. no. Besides, in Table 10 the authors use BMI and obesity in the same model which is questionable due to co-linearity. Besides, I don’t understand how obesity is defined (the rate is low according to Table 2); I would prefer that BMI>30 is used as obesity. As regards age and mean arterial blood pressure, these are used as continuous variables I presume, and this should be mentioned. As regards some of the non-linear variables in association with HRQOL, e.g. duration of hypertension or symptom count, could be presented as different variables in order to solve some of the problems (e.g., if medium duration is reference, lower and longer duration, respectively, could be compared to this).

Response: All the suggestions in this comment have been effected in their merit. The non-linear variables have been dichotomized and the co-linearity between obesity and BMI has been resolved by defining obesity as BMI>30.

Minor:

4. Title: Should preferably include Nigeria.

Response: The authors believe the study is from a tertiary health centre in South western Nigeria and not representative of Nigeria, and since the study location is stated in the methods may not be necessary in the title.

Results and tables: the authors use many tables, and some of them could be merged into one, e.g. Tables 1 and 2. In Table 2 both numbers and percentage could be given, as in Table 1. When reporting results of HRQOL I prefer one decimal; two decimals make no sense. In the multiple regression the authors use three decimals which is acceptable. The text to the tables is sparse, and this makes the tables hard to interpret. See also remarks on statistics above!
**Response:** This correction has been done.

5. Discussion: the first paragraph is very important, and I totally agree that results in hypertensive subjects in developing and developed countries could actually differ.
One important issue to discuss is whether the perception of disease and therefore the assessment of HRQOL differs between countries and cultures, and the authors should mention and discuss this. They mention differences between their own sample and a population based sample in Sweden; could their results be related to other samples? Do they have comparison samples from Nigeria, or other African countries?

**Response:** We have tried to discuss the implications of the present study. Maybe for future study we will be able to look into comparison from different samples in Nigeria and beyond.

**Level of interest:** An article of importance in its field  
**Quality of written English:** Acceptable  
**Statistical review:** Yes, and I have assessed the statistics in my report.  
**Declaration of competing interests:** I declare that I have no competing interests

The authors believe that all the issues raised have been addressed and wish to thank the reviewer for doing a splendid job.

Mr. Ogunlana M.O

Principal Investigator.