Reviewer's report

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

Version: 2 Date: 19 February 2009

Reviewer: Per Wändell

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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.

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.

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).

Minor:

4. Title: Should preferably include Nigeria.

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!

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?

**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