Reviewer's report

Title: Prevalence of Hypertension and Associated Cardiovascular Risk Factors in an Urban Slum in Nairobi, Kenya. A population-based survey

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Reviewer: Andre Pascal A Kengne

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MAJOR COMPULSORY REVISIONS

The authors have submitted a revised version of the manuscript above.

1) I don't know why the colleagues are choosing to be more dismissive, instead of using the reviewers’ comments to improve their work. The suggestion by the authors that the WHO STEP questionnaire was not designed to capture data on hypertension detection, treatment and control rates is very shocking. It can relate to anything else, but not the STEPs questionnaire. Questions 68-70 of the STEPS are about the history of diagnosed hypertension (and will provide data on the detection rate); Questions 71-73 are about on-going treatments for hypertension including lifestyle measures and will provide data for computing the treatment rate; while the BP levels measured during the survey will provide information on the levels of BP control among participants on treatment.

2) The authors further argue that their purpose was to determine the prevalence of hypertension, screening rates and correlates of hypertension. Are the authors really expecting to uncover correlates of hypertension different from those largely described elsewhere and using more robust approaches such as cohort studies? It is already known and widely admitted that the prevalence of hypertension is high and rapidly growing across Africa. Against this background, new studies on hypertension prevalence in this setting cannot afford not describe in details the detection, treatment and control rates and where possible their determinants. Only these aspects are expected to vary across setting, and by investigating them, the authors provide context specific evidence for action. I shouldn't appear rude by suggesting that any focus on the prevalence of hypertension and correlate only is of little use. Reference 20 to 23 give some examples the authors could get inspiration from.

3) Perhaps the authors should be more specific about what screening/intervention targets they feel could be informed by their linear regressions models to determine the predictors of absolute SBP and DBP levels. What’s novel from those analyses?

4) If the authors feel that they should maintain the many tables, perhaps they should strive to present the full statistics everywhere.
a. All the p-values showing 0.000 should be presentment as <0.001

b. In table 1, the p-value for comparing the age strata are also needed. Furthermore both in this table and everywhere else, there is a misuse of IQR (interquartile range). IQR is a single number representing the distance between the 25th and 75th percentiles. I think currently the authors are showing the 25th-75th percentiles and should label it as such or Q1-Q3. The parenthesis should be detached from the proceeding numbers.

c. Table 2: a single p-value should compare men and women across BMI categories and not 4 p-values as currently done; this also applies to age groups comparisons in the online tables. This table remains of no utility for the current study (just like the online only tables) as the same data in major way can be derived from Table 4.

d. Table 3: the p-value or men vs. women comparisons are also needed

e. Table 4: the way comparisons are done is really not capturing the most useful information from the data. At most the authors are comparing men and women within specific strata. Taking BMI as an example, what is of interest is whether using Normal weight as a reference, overweight and obesity are associated with higher risk of Hypertension. The useful OR therefore here are the OR for overweight vs. normal, and Obese vs. normal weight. This applies to other factors in the table. What is of interest is the risk across levels of the factors and not the risk across gender within each level of a given factor. For this, perhaps the authors should show only the age and sex-adjusted OR, and the multivariable adjusted OR. I can’t see why this multivariable adjustment model shouldn’t include BMI categories, diabetes status, physical activity and smoking.

5) In the discussion, I am still of the opinion that the 3rd paragraph has little value, and if the authors still want to maintain it, perhaps they should review their statement on the superiority of central obesity over general obesity in determining the risk of hypertension. Strictly speaking, they haven’t compared the predictive utility of BMI and WC for hypertension risk in this study. Because of the high collinearity between BMI and WC, it is an expectation that one may cancel the effect of the other in the same multivariable models. This doesn’t imply that one is superior to the other.

6) Other comments.

a. The reference of the authors to the use of ANOVA and KW test to assess the trend in the method section is inaccurate. They didn’t assess the trend in this study and I am sceptical about the applicability of the listed tests for assessing trends.

b. They authors may want to criticise the methods for quality control of theirs instruments in the limitation section of their paper.

**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:
None