Reviewer's report

Title: Malaria patients' satisfaction with life in the context of their trust in doctor/nurse and the level of acceptance of the disease: a quantitative study.

Version: 1 Date: 1 June 2013

Reviewer: Elizabeth Ekirapa-Kiracho

Reviewer's report:

Discretionary revisions “Minor issues not for publication”
1. Title: The title as it reads now, is not clear, precise nor is it focused. The words quantitative study looks redundant. My suggestion for a title could be: Acceptance of illness and satisfaction with life among malaria patients in Rivers State, Nigeria

2. The second paragraph of the background section has a couple of long sentences which need revision so the message is clear. (For example the sentence which begins with In analyzing a patient’s.........).

3. Although the study objective was clearly given, the gap in knowledge that study tried to address is not clearly articulated in the background especially in Nigeria.

4. Under the subsection satisfaction with life, the authors write that the mean level of SWL was approx 16.5. I think the word approximately is not necessary since the mean is already an estimate. Secondly, when summarizing results using measures of the central tendency, you do not have to describe a variable using both means and medians at the same time. Choose one and justify your choice. Generally the median is a better estimate for highly skewed data. You could explore your data and assess the degree of skewness thereafter come up with the best statistic to describe SWL.

5. Under the sub section “quantitative research methods” in the methods section, the authors mention various scales that were used to measure their outcomes and how grading/scoring was done on the likert scale and this is good. They could consider including the different themes or questions which were scored under each scale. This would put into perspective, what was actually scored hence judgments could be made on the relevancy or adequacy of such scales.

6. Specifically on the Acceptance of illness scale (AIS), the outcomes were categorized into: no acceptance of the disease, moderate and good acceptance. The basis for this classification should be given or atleast a reference should be given if such classification has ever been used previously.

Minor compulsory revisions
1. Authors say that the study was done on patients with malaria, they don’t tell us how malaria was diagnosed given that there are different methods of diagnosing malaria and these tests have different specificities and sensitivities. Was it the
same method of diagnosis? Any validation?

2. The 3rd, 4th and 5th paragraph of methods section under study area and study population actually contain results, I recommend that these paragraphs be transferred to the results section. “Minor issues not for publication”

3. The 1st paragraph under the discussion section has information that is suitable for the back ground section. Similarly, the first 3 paragraphs under patient physician trust scale contains material that could be more relevant for the background section. The 4th paragraph could be shifted to the methods section.

4. The third paragraph of the background section comprises of one very long sentence this could be broken into several sentences. “Minor issues not for publication”

5. In the fourth paragraph last sentence ( starts with Health condition is one ……. ) delete letter l that has been added to the – this currently reads as lthe limitations ….“Minor issues not for publication”

6. Under the quantitative research methods section in the second last paragraph clarify whether the questionnaires were interviewer administered or self administered, this is not clear. “Minor issues not for publication”

7. In the first paragraph under the sub title statistical analysis in the Methods section correct the grammar of the 3rd sentence ( For each scale…….) and 5th sentence (If there were any rules ……..)”“Minor issues not for publication”

8. In the third paragraph under statistical analysis correct the grammar of the sentence ( For the determination of the diversity…….). The meaning is not clear. The sentence is also rather too long.

9. In the last paragraph under the subtitle Patient physician trust scale (in discussion) the authors report that their results are similar to those of Chilika who did work among obstetric and gynecological patients. It would be useful for the authors to also discuss if any differences could be expected from the trust of physicians in patients with malaria, compared to the trust of physicians in patients with obstetric and gynecological conditions.

10. In some places the authors refer to literature data this should be corrected and data deleted – see 2nd paragraph under sub title Correlations between selected scales ( in discussion ) “Minor issues not for publication”

11. In the 3rd paragraph under the results section, authors say that results were transformed into the adjacent scale, but they don’t explain clearly how the transformation was done, the categories they got plus the justification for such classification. Perhaps they could provide a reference where such categorization was used before.

12. The last paragraph under the sub section “correlations with selected scales” (Trust in personnel and acceptance of illness and satisfaction with life) should be transferred to the discussion section since what was put in this paragraph was not a finding of this study.

13. In the second paragraph of the background section, most of the sentences

14. The second paragraph under the sub section satisfaction with life scale (SWLS) is too long. It could perhaps be summarized as follows: About a third (37%) of the respondents scored above 17 points while the rest scored less than a half the maximum score on the SWLS. The 1st paragraph under the subtitle patient physician trust scale could be summarized as: the mean score from the the patient - physician trust score was 50.6 (S.D+ 3.8) and details are shown in table 3.

Major compulsory revisions
1. The figures and tables are rather too many, they should be reduced. Since some results are already presented in text some of the tables and figures may not be necessary.
2. In the background section, more information could be provided about malaria statistics, and malaria endemicity (besides mentioning that P falciparum is the commonest cause of malaria), as well as the health care/system in this part of Nigeria: these potentially influence the patient-doctor interactions thus quality of life and satisfaction with life. The background section could also provide some information on what exists in the literature about acceptance and quality of life among malaria patients.
3. Still about the study population, the eligibility criterion (inclusions and exclusion criteria) is not mentioned yet this is important in such studies.
4. Authors say that 140 respondents/patients were studied yet the considerations and assumptions of how they arrived at this sample are not mentioned. These are important since they not only predict the power of the study but also the extent of external validity (the degree to which results are generalizable).
5. In relation to the above, the authors say that about 30 - 60 patients were interviewed each day and it is not clear how these were sampled (whether systematic, or simple random or exhaustive sampling was done). It's not also clear how doctors and nurses were selected (key informants?).
6. Perhaps the authors could justify the choice of statistical tests (Man-Whitney and Kruskal-Wallis test (Was is it because of small sample size or none normality of their outcome variables at population level?)
Results

7. Score categories reported in the SWLS and the patient physician trust scale are overlapping. For example authors say: 3% scored 5 – 8, 1% scored 8 – 11, 21% scored 11 – 14.

8. The sub title Social demographic and values of measuring scales under the results section, is not clear please modify it. The way the results about the influence of gender, age, marital status and professional status are presented is also rather confusing. This should be improved. Under marital status, the authors say that marital status affected the level of acceptance of illness, but they don’t tell us which categories under marital status was more or less likely than others to accept the illness. Under professional status more categories could have been looked at other than classifying this as employed and unemployed. Such categories could include: peasants, artisans, students, housewife, casual laborer etc.

Discussion

9. The discussion could benefit from some restructuring. I suggest that the authors, start with a paragraph that summarizes the results, giving the broader picture of this study (main findings), but in the absence of interpretation. Basically in the first paragraph readers should find a summary of what was done and found (satisfaction with life and acceptance of illness). In the following paragraphs, the authors should then discuss and interpret their findings in relation to other studies whether in agreement or not. Perhaps also give reasons for such differences and similarities (sometimes these could be due to contextual or methodological variations). For each major finding, the author should give the policy implications after which you mention the limitations and finally the conclusions.

10. In the section under the title acceptance of illness scale (in discussion), could the authors discuss the influence of severity of malaria including complications in the acceptance of illness. Did the authors classify the level of sickness (malaria severity) perhaps this could have been more informative in relation to satisfaction with life? If this was not dome perhaps it could be mentioned as a limitation.

11. In several places, the authors quote work that is not directly related to the topic of study, I suggest that these sections are deleted and more appropriate literature cited. See sections below for details
- Under the subtitle Satisfaction with life scale (in discussion), the authors cite work done by Opiyo and others in Kenya relating malaria control and saving resources for family enterprise. In my view this citation is not appropriate here since the authors are discussing different themes altogether.
- Under the section Patient nurse trust scale (in discussion) results from Poland about attitudes towards nurses, and value attached to their services is mentioned. However it has not been linked appropriately to the topic – patient nurse trust. This could be improved upon.
- Under the subtitle social demographic and values of measurement scales (in
discussion) several authors are quoted however their work is not directly related
to the topic of study.
# Work by Ojakaa and Xu et al is quoted. This work relates mainly to decision
making in families and not to the influence of gender and trust in the physician.
# In the same section work by Opiyo on knowledge about malaria is quoted.
# Tipke et al are quoted, but their work is about determinants of availability of
antimalarials
# Work in Ethiopia about delay in seeking malaria treatment
# Work by Chuma about quality of malaria services

12. It's often good practice to include recommendations from the study under the
conclusion’s sub section. These could be included.
13. The following should be added to the study limitations. The authors should
consider the following issues and add them to the relevant sections or include
them as limitations.
• Extent of generalizability of their results
• Pre testing of the survey questionnaire and effects on validity of the tools.
• Translation of questionnaire into the local language. This could have been okay
if the study population was from a selected group with high literacy. However in
this case, the results could be generalizable to only similar populations and not to
areas with low levels of literacy.

Level of interest: An article whose findings are important to those with closely
related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a
statistician.

Declaration of competing interests:

I declare that I have no competing interests