Reviewer's report

Title: Utilization of ITNs during pregnancy among postpartum women in Ibadan city, Nigeria

Version: 1 Date: 21 September 2011

Reviewer: Jennifer Unger

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Review of: Utilization of ITNs During Pregnancy Among Postpartum Women in Ibadan City Nigeria

Authors: Joel Aluko and Olanihun Oluwatosin

Reviewer: Jennifer Unger

Major Compulsory Revisions:

Aluko and Oluwatosin address an important question in their study regarding the utilization of ITNs during pregnancy. However, the current manuscript does not clearly present the work performed and the potential contribution to the literature. The authors state “this study sought to assess the utilization of ITNs during pregnancy among postpartum women in Ibadan” which is of interest to those living or working in endemic areas. However, the manuscript does not maintain focus of this objective and fails to highlight some of the most important findings associated with this objective.

Major Compulsory Revisions:

Background:

Paragraph 2: The authors state that “Pregnancy is known to worsen malaria by depressing the immune system....” They cite a work from Okwa as the basis of this statement. In fact, pregnant women are at increased risk of symptomatic malaria because of distinct malarial parasites that bind to the placenta (in the case of Plasmodium Falciparum). The immunosuppression caused by pregnancy may be a factor but is not supported by the paper cited and is not been well established by other studies. Primigravidas and their infants are at the greatest risk of complications from malarial infection because they have not developed immunity to these placenta parasites. Over subsequent pregnancies, women do acquire partial immunity to the pregnancy associate parasites. Thus in paragraph 3 when the statement “Although Nigerian pregnant women have acquired immunity due to the high endemic and transmission rates.....” is made by the authors, it would be important to explain why these women are susceptible despite the immunity. In other words, pregnant women usually HAVE developed immunity to most parasites except the placental form which they never encounter until they are pregnant.
Paragraph 5:
The authors state that their study is innovative because, in comparison, the NDHS only reported on pregnant women during various stages of their pregnancy. While it is true that this current study gives a different “point of view”, looking back at the entire pregnancy that does not support the statement that the NDHS “failed to reveal the exact utilization pattern”. Perhaps it failed to characterize utilization during the different stages in pregnancy which does have some clinical relevance but this statement should be more specific to the unique question of the current study – what new information does this study aim to find?

Paragraph 6-7:
The authors attempt to explain the malaria prevention model based on Bandura’s Social Cognitive Theory. The transition and explanation are confusing and difficult to follow. This needs to be simplified and perhaps referenced instead of distracting the reader from the main focus of the study. It is difficult for me to understand the model that was used and how it was used. The authors state that “The malaria prevention model adapted from SCT as employed to guide the study” but I do not think they are actually testing the SCT as an intervention or the effectiveness of this model. Therefore it is unclear why they are suggesting it as a more effective model of ANC. This approach may be more effective but it appears out of place in this manuscript.

Methods:
Paragraph 1: The authors should discuss the validity of the questionnaire since they made the comment that it was validated. Overall the methods are not as clear as they could be. This was essentially a cross-sectional study to assess the utilization of ITNS in pregnancy among these women. This should be stated clearly and then the authors should discuss the questionnaire, sampling, population etc.

Paragraph 2: The authors should describe the random sampling.

Paragraph 3: The level of knowledge regarding MIP and attitudes toward ITNS is very interesting. It would be important to describe these scales and their validation. Are these published scales? Could they be included in the manuscript?

Results:
Overall there is too much detail in the results also presented in the tables and figures. The authors could summarize any interesting demographic findings and refer to Table 1. The most interesting and relevant parts of the study can be found under “Awareness and utilization of ITN”, “Knowledge”, “Attitudes”. These should be the main headings in the results section. There is a great deal of repetitive data in the results. For example: under “Awareness and utilization”: The statement “Similarly 228 (68.1%) had seen ITNs, while (32%) had never…” and this adds up to 100.1%.
The CHI SQUARE tests were done to assess associations though they only reported those with a relationship.

Hypotheses Testing:
I understand that these associations were found, however: where these hypotheses made prior the study? What is the relevance of religion and use of ITN? If the authors chose these hypotheses prior to the study it would be important to describe why these were chosen. If these are the findings of significant associations found after study completion than they should not be hypotheses and should be presented with the results. The authors also need to describe the purpose of this analysis ie: In order to better characterize those women likely to own an ITN we analyzed the data for ....

I would consider adding these to the results in a chart: women who own an ITN versus women who do not.

Discussion:
This section can be largely trimmed. There are statements that are too obvious for this manuscript. They do not need to make statements about teenage pregnancy unless there is something significant about it. Nor do you have to state “Women below 20 years and those above 35 years are not usually as many as women aged 20 to 34". It is probably enough to describe the average age, say this is representative of pregnant women in the country and leave it at that.

Overall there is too much justification of the result in the discussion. The main purpose should be to support the findings under the main objective of the study. Therefore the authors could summarize the first two pages in the discussion to make a statement about this population being representative of the general pregnant population in Nigeria.

Starting on page 12 – The awareness of ITN’s among the participants was very high. The authors appear to make the following important points: Women know about ITNS. They learn about ITNs from ANC. However, women do not own ITNs and have not received them from the free distribution programs. Finally women do not like sleeping under these ITNS bc they are uncomfortable. The authors need to discuss these significant findings in the context of what one would expect given the government programs or ANC attendance. They do this better in the abstract.

The authors spend a good deal of time documenting previous studies in the discussion with regards to subject’s demographics. This time might be better spent discussing their findings and how it compares to other studies both inside and outside of Nigeria (good studies have been carried out in Ethiopia and Kenya).

The study is novel in that it allows examination of entire pregnancies. It also addresses the important fact that by the time women present to ANC they have already contracted pregnancy malaria and it has already infiltrated the placenta.

The limitations of the study are not fully explored. This is a cross sectional study relying on recall of subjects. The study was designed to be cross-sectional so #2
is not actually a limitation.
The authors should incorporate their conclusions into the discussion. Again I
would like to see the knowledge and acceptance scales to determine if these are
limitations.

Minor Revisions:
The manuscript needs to be reviewed for grammar. I have only documented a
few necessary revisions.

Abstract:
1.) Would not use word attack: substitute “malarial infection” for “malaria attack”.
Also, do not think that this first sentence is substantiated by the data.
2.) The effective use of ITNS would be of BENEFIT to these vulnerable women.
3.) The semi colon in the fourth sentence is misused.
4.) Under methods: and “are known” for provision of care under…
5.) The first sentence of the conclusion on the Abstract is too dense. Need to
separate out those important points.

Introduction:
Page 4: Pregnant women are one of the key target groups of RBM
The four key intervention strategies of RBM are: case management, USE OF
sulfamethoxazole (SP)
Therefore this study sought to assess the utilization of ITNS DURING
PREGNANCY by post partum women. This is important bc the whole point is use
in pregnancy.
The model illustrates how a pregnant WOMAN’S functioning is viewed.

Page 5: Information elucidated includes…

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

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.