Reviewer's report

Title: Do consumers' preferences for improved provision of malaria treatment services differ by their socio-economic status and geographic location? A study in southeast Nigeria

Version: 1 Date: 23 July 2009

Reviewer: Patrick Kachur

Reviewer's report:

Thank you for the opportunity to review this interesting manuscript.

Please excuse my tardy response. I neglected my review while traveling and only now realized it. I am unable to recommend this manuscript for publication in BMC Public Health until the following recommended major compulsory and minor essential revisions are addressed. My general and detailed comments to the authors appear below.

GENERAL COMMENTS

1. Is the question posed by the authors well defined? Yes.
2. Are the methods appropriate and well described? More detail in description of the methods is needed.
3. Are the data sound? To the extent that the relevant data were collected and analysed they appear sound. It is unfortunate, however, that only 2 risk factors are considered.
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Many key elements describing the methods of sample selection, data collection and analysis are not present.
5. Are the discussion and conclusions well balanced and adequately supported by the data? No.
6. Are limitations of the work clearly stated? No.
7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? This could be done more completely 8. Do the title and abstract accurately convey what has been found? No abstract is present.
8. Is the writing acceptable? Yes.

DETAILED COMMENTS

- Major Compulsory Revisions
1. page 1. The authors should provide an abstract of their paper prior to the introduction and background section.
2. page 3 paragraph 1. Describe how the LGAs and communities within them were selected for this study. Were these done in systematic or representative ways such as a selection proportional to population size? If not, how might that affect the ability to generalize to the whole of the LGA, or Anambra State, much less beyond.

3. page 3 paragraph 2. The authors do not state the parameters around which they calculated the sample sizes. What was the primary outcome of interest that drove their power calculations and what was their estimate of prevalence? In addition although the authors state that the households in each site were selected 'randomly', they fail to provide any indication of the basis on which this selection occurred.

Were households enumerated before selection? If so, how and by whom? Where missing or unconsenting households substituted? If so, according to what protocol.

4. page 3 paragraph 3. more information about how the ranking was done and the rating would be helpful. This creates a bit of confusion throughout the paper whenever the terms ranking and rating are employed...For example one could state: ‘Participants were given a list of different sources of treatment (home, public and private hospitals, public PHC centres, pharmacy shops, patent medicine dealers, trained mothers, herbalists and community health workers). They were first asked to rank the 3 they most preferred then rate each treatment source from 1 to 10.’ It will be essential to review the rest of the ms closely to make sure that the terms rate and rank are consistently used to reflect data collected from these precise steps.

Finally it's not clear whether the ranking allowed participants to state relative preferences among the top 3, or if all 3 sources mentioned were ranked equally. Nor is it clear whether the rating from 1-10 was mutually exclusive and required that each score be used once and only once (in which case it might more accurately be describe as a ranking exercise), or if multiple options could be scored at the same level of preference.


5. page 4 paragraph 2. The authors state that "Principal components analysis (PCA) was used to generate the SES index." This is an appropriate and standard method, but it would be helpful to provide the reader with the list of components that contributed to this index and some explanation of how the resulting index was distributed.

6. page 5 paragraph 4 and table 4. How were chi-square significance tests produced around the equity ratios?
7. page 8 paragraph 2. the authors conclude that because their ranking and rating scales produced similar results their "...findings are valid and really represent well thought-out preferences." While it does appear that their ranking and rating exercises produced similar results, it really only shows that they are 2 ways of measuring the same thing and likely to be prone to the same biases. This does not, however, convince the reader that either of the measures would be internally or externally valid as measures of preference in a more naturalistic setting.

8. page 8 paragraph 3. the conclusion that "preferences...were influenced mostly by SES and also by geographic location..." is deceptive, since these are the only 2 risk or predictive factors measured in this study. It is entirely possible that other unmeasured factors weigh much more heavily.

- Minor Essential Revisions
1. page 1 paragraph 1. The authors should specify the specific "Abuja meeting" to which they are referring.
2. page 3 paragraph 3. the authors state "...questionnaire was used to obtain information from randomly selected respondents." However, they clearly define the household as the unit of selection. Therefore they obtained information from respondents in randomly selected households.
3. page 5 paragraph 4 and table 4. The approach of calculating chi square statistics around the equity ratio between the first and last quartiles of SES seems to exclude the second and third quartiles, i.e. at least half of the data. The authors should consider whether or not including SES as a continuous variable and calculating a test for trend would make more complete use of the data collected.
4. pages 5 and 6. The statements at the very end of the results section about a rural preference for herbalists and public hospitals don't seem well supported by the data in the tables. I wonder if the authors didn't wish to point out the slightly higher ranking or rating of private hospitals among urban respondents. At any rate the numbers here are extremely small and probably don't support these generalizations.
5. page 8. somewhere prior to arriving at conclusions, the authors should present clearly the potential biases inherent in their sampling decisions, data collection approaches and analyses.

- Discretionary Revisions
1. page 3 paragraph 3. the authors may wish to consider the extent to which recruiting local educated residents of each community as field workers may have affected the potential for bias in reported responses.
2. page 5 paragraphs 2 and 3. it may be helpful to present the results of the ranking exercise ahead of those of the rating exercise...that would make it consistent with the description in the methods section and with the order in which data were collected.

3. page 7 paragraph 2. the statement "Herbalists must have been viewed as inferior goods, where demand fall as income increases as was alluded to by their low preference as SES increased" is not clear. It appears to conflate income (NOT measured in this study) with SES (for which a proxy was measured), as well as demand (NOT measured) with preference (measured in a hypothetical condition).

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

**Declaration of competing interests:**

- I declare that I have no competing interests.