Author's response to reviews

Title: Attitudes towards Mental Illness in Malawi: A cross sectional survey.

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Author's response to reviews: see over
To,

The Editor

BMC Public health.

Dear Editor,

Re: Submission of revision, “Attitudes towards Mental Illness in Malawi: A cross sectional survey”.

We apologise for the delay in submission of the revision. We are extremely grateful to the reviewers for their comments and suggestions. Our research team has considered their comments and we have made every effort to incorporate all of their suggestions into the revised manuscript. We are of the view that their thoughtful and constructive criticism has strengthened our work.

Below is a point by point response to each of the reviewers’ comments as well as a response to the overall editorial criticism. For clarity we have written our responses in blue font.

We very much hope that our revised work will be considered suitable for publication in BMC Public Health. We would of course be delighted to make further amendments if felt necessary.

Yours Sincerely

Dr Jim Crabb

On behalf of the authors.
Reviewer 1:

Though the issue of stigma of mental health has gone beyond knowledge and attitude, the importance of the issue of stigma, especially in Africa, will make any research that can deepen our knowledge worthwhile. The article is well written.

Major Compulsory Revisions

The authors may do well to include in the introduction a description of the state of mental health services in Malawi so as to provide readers a context.

Author response: A paragraph on this has been added to the end of the introduction.

They also need to describe the epidemiology of mental disorders in Malawi and use this to strengthen the rationale for the research.

Author response: unfortunately there is little up to date epidemiological data regarding mental disorders in Malawi. What is known is summarised in the last paragraph of the introduction.

Authors should emphasize very clearly that their study is inherently different from the Gureje study from Nigeria in terms of sampling method, coverage and sample size. This should be sighted clearly as a major confounder for the comparison with their study. For instance, their explanation for the differences in the view of the Malawi and Nigeria participants on the issue of brain disease as a cause of mental illness did not appear to take into cognizance of the fact that an hospital based sample could have received health education in addition to their own beliefs. This may also explain the contradiction that the authors alluded to.

The Malawi participants could have endorsed both spiritual and brain disease
causation of mental illness in one single breath if the brain disease model had
been sold to them in the hospital on top of their own held beliefs. It may reflect
ambivalence. This, authors will agree, is not the case for the unalloyed
community sample from Nigeria.

Author response: These concerns have been addressed in paragraph 1 & 3
in the discussions section.

I feel authors misinterpreted the question that inquired if mental illness could
be treated in the hospital. The question is in the hospital and NOT in this
hospital. The idea is if hospital care is appropriate for mental illness and not if
the hospital where study took place is appropriate. So, in my own view, this
study have only shown that majority of respondents believed that mental
illness could be treated in an hospital setting and not necessarily in the hospital
where the study took place as the authors seems to view the response of
participants . Being an hospital based study, those with a positive view of
hospital care for mental illness has been inadvertently selected. Therefore, most
of the arguments of the authors
in that regard are faulty and should be re-framed.

Author response: we agree with the comments of the reviewer. Our
argument has been reframed as suggested on page 12, paragraph 2. We
feel that our observation that a bias in favour of hospital treatment may
reflect the reality that there is essentially no alternatives to this in
Malawi is still valid and worth making.

Minor essential revisions

It will be nice to state the reason (if known) why the three participants
decided.
Author response: unfortunately we do not have this information. The consent form used in our study states explicitly that an individual can refuse consent at any time without having to give a reason. This is standard practice and we doubt our study would have been granted ethical approval if we had specified that individuals had to give a reason for not taking part.

Discretionary Revisions

Authors may need to know that there is no uniform definition for stigma, so the phrase.."can be defined" is better than.."is defined"

Author response: this has been changed

It may also be important to add that stigma is also a major reason why sufferers of mental illness fail to acknowledge their illness (Rockville, 1999) and the underlying factor mitigating against social re-integration of persons recovering from mental illness (Klin and Lemish, 2008).

Author response: these are very helpful references. They have been added to the first paragraph of the introduction.
Reviewer 2:

This is a potentially interesting manuscript that describes attitudes towards mental illness in Malawi, a country in sub-Saharan Africa about which there has been little published in the mental health field. Data were gathered via a cross-sectional survey of patients and carers attending various outpatient clinics at a tertiary teaching hospital in the second largest city in Malawi.

Major compulsory revisions

As it stands, however, there are a number of major problems.

The methods are unclear – are the data from researcher-administered interview, a self-report questionnaire completed by patients or their carers, or both?

*Author response: This has been clarified in the first paragraph of the methods section sentences 2-5*

It is also not clear how the measure of stigmatising beliefs used in analysis was derived – the sums of all or some of the questionnaire items?

*Author response: This has been clarified. Second last sentence in the methods section*

Reporting a power calculation seems to make little sense since this is not an intervention study. The sample size of 210 (132 patients and 78 carers) is more than sufficient for a descriptive study like this.

*Author response: The paragraph justifying the sample size has been removed*
The results are poorly presented, especially Figures 1-3. I would prefer such data presented in a table, possibly with two columns – one for those attending the psychiatric and epilepsy clinics and one for those attending the other clinics.

Author response: the data from figures 1,2 & 3 is also in table form, please see table 3. With regards to having separate columns relating to psychiatry/epilepsy clinic responses and non mental health clinic responses- there was only a significant difference found in responses from the 2 groups of clinic attenders on a single question. On all the other 16 questions there was no statistically significant difference in answers between those attending psychiatry/mental health and non mental health related clinics. This is an important finding. We are grateful to the reviewer for highlighting this and the point has now been made more clearly in the results section. We would respectfully venture that including a large amount of non significant data as extra columns in the main table of results would not add to the paper, indeed this may lead to the table, and therefore the results being more difficult to interpret.

In the text, percentages are preferable to phrases such as “a reasonably equal distribution”.

Author response: Percentages have been added throughout the results and discussion section as requested.

I would also prefer to see the breakdown by source (psychiatric/epilepsy clinic vs. other clinics and patients vs. carers) before the breakdown by demographics and other personal data (gender, education etc).

Author response: This has been addressed as requested. See table 1 & table 2.
The discussion would be better separated from the results, possible starting with the last paragraph on page 8. (Page numbering does help.) This whole section seems overly long with much unnecessary material. For example, the second sentence in that paragraph could be deleted and nothing would be lost.

*Author response:* The discussion has been separated from the results. Page numbering has been inserted. The paragraph has been considerably shortened. Instead of describing the results the reader is asked to refer to the table.

In the discussion of limitations there was only one that I could see – lack of generalisability of the results to the general population, or other sub-populations, given the selected sample.

*Author response:* the other main limitation of our study is that we are unable to ascertain why participants held certain views or attitudes regarding mental illness. This is a limitation of the qualitative design. We have therefore qualified our observations and conclusions and have recommended that further qualitative work be performed. Please see last paragraph before conclusions section.

A major conceptual problem throughout (from abstract onwards) is the interchangeable use of “spirit possession” with “spiritual causes”.

*Author response:* Spiritual possession refers to the specific question asked of participants in our study. Spiritual causes or attributions meanwhile refers to the 2 questions (mental illness being due to gods punishment and spirit possession). This distinction has now been made explicit throughout the text.

Minor essential revisions
There are also a number of minor problems. Inconsistent use of hyphens, e.g. sub-Saharan, cross-sectional.

*Author response:* This has been addressed as requested

The term “subjects” seems a little old-fashioned; “participants” would be better.

*Author response:* This has been addressed as requested

The persons accompanying the relative in the waiting room are variously, and confusingly, described as “relatives”, “carers” and “guardians”.

*Author response:* This has been done as requested

The title is short and suitable informative but the body of the text could be considerable tightened. A good edit would pick up “counter part” on page 8 and "Attitide" on the vertical axis in Figure 3 as well as other linguistic and typographical errors.

*Author response:* this has been done as requested
**Additional editorial requirements**

Please make the following formatting changes during the revision of your manuscript. Ensuring that the manuscript meets the journal’s manuscript structure will help to speed the production process if your manuscript is accepted for publication:

(1) The figure file should not include the title (e.g. Figure 1... etc.) or the figure number. The legend and title should be part of the manuscript file, given after the reference list. Please ensure that the order in which your figures are cited is the same as the order in which they are provided. Every figure must be cited in the text, using Arabic numerals. Please do not use ranges when listing figures. For more information, see the instructions for authors:

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*Author response: This has been addressed as requested*

(2) Please ensure that the order in which your tables are cited is the same as the order in which they are provided. Every table must be cited in the text, using Arabic numerals. Please do not use ranges when listing tables. Tables must not be subdivided, or contain tables within tables. Please note that we are unable to display vertical lines or text within tables, no display merged cells: please re-layout your table without these elements. Tables should be formatted using the Table tool in your word processor. Please ensure the table title is above the table and the legend is below the table. For more information, see the instructions for authors on the journal website.

*Author response: This has been addressed as requested*

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