Author's response to reviews

Title: Comparing the accuracy of brief versus long depression screening instruments which have been validated in low and middle income countries; a systematic review.

Authors:

Dickens H Akena (akenadickens@yahoo.co.uk)
John A Joska (john.joska@uct.ac.za)
Ekwaro A Obuku (ekwaro@gmail.com)
Taryn Amos (tarynamos@gmail.com)
Seggane Musisi (segganemusisi@yahoo.ca)
Dan J Stein (dan.stein@uct.ac.za)

Version: 3 Date: 26 September 2012

Author's response to reviews: see over
Reviewer's report # 1:
The paper is certainly improved and deserves to be published.

1. However, I suggest a more careful revision of typos throughout the paper e.g.: in some cases the authors left a space between words and parentheses whereas in some other they did not. I am not aware if it is intentional.

   *We have used a uniform alignment of the manuscript. This we believe will make the spaces in between the words uniform.*

2. The reference section should be revised since it seems very "heterogeneous" about the style adopted.

   *We have used the end note software to insert the references, according to the BMC style. However, we will be glad to seek more guidance about which references are not correctly inserted from the editorial office. We believe that we can work with the editorial office to improve on this.*

Reviewer's report # 2:

Response 1
I accept the inclusion of pregnant / postnatal women. I recommend including the sample type (an extra column) in Table 1 to aid interpretation of the generalisability i.e. whether male/female, pregnant, postnatal clinic, delivery ward, primary care, HIV facility, general medical in-patients, and general medical out-patients). I note that the Stewart (Malawi) validation was conducted in postnatal women – infants 10 months old. It would be good to highlight this.

*Table one has been split into two. The first table is a descriptive summary of included studies including study site, study population.*

*The second table has the prevalence’s, and the information that was critical in assessing for heterogeneity.*

Response 2
As I noted previously, the Tesfaye study did use MDD as criterion (and presents ROC curves for minor and major depression). I believe it should, therefore, be included.

*The Markos Tesfaye study has been included; indeed as reported in the results section (3.3.5), the K-6, K-10 and EPDS were used to identify both minor and
major depression. However, the inclusion of these three scales didn’t affect the statistical outcome of the analysis. Still significant heterogeneity was observed. We thank the reviewer immensely for bringing this critical detail to our attention.

Response 5
The new point about weighting is not clear to me. Could additional explanation be added, perhaps in the methods?

The weighting had been included to explain the contribution of each study to the analysis (often useful in a meta-analysis), and was meant to elucidate what was appearing in figure two. However, since we are no longer reporting our findings as a meta-analysis, we have deleted the weighting which is indeed meant to explain the findings in a meta-analysis.

Response 6
I still recommend including the point about ultra-brief screens not including questions about suicidal ideation as a potential disadvantage.

The issue of ultra-brief scales has been addressed in two sections, the conclusion of the abstract and the conclusion of the main body of work. We have included the fact that these scales don’t encompass the whole range of depression symptoms and should be followed up with detailed psychiatric interview.

I am concerned about the interpretation that there is no significant difference between brief and longer scales. Due to the heterogeneity you were unable to conduct sub-group analyses so I don’t know how this statement can be justified. See below.

The interpretation of significant differences relied on having conducted a meta-analysis. However, since this hasn’t been the case, a number of statements including ‘statistical significance’ have been removed from the abstract; such statements became redundant.

Response 7
There are still problems with the references. I leave it to the editorial team to liaise with you about this.

We have used the end note software to insert the references, according to the BMC style. However, we will be glad to seek more guidance about which references are not correctly inserted from the editorial office. We believe that we can work with the editorial office to improve on this.

New comments that are essential to address (major compulsory revisions)

It is inconsistent to say that you identified substantial heterogeneity and then go on to present a pooled estimate (and also to conduct sub-group analyses). With substantial
heterogeneity the implication is that a meta-analysis cannot be carried out. Therefore I think the pooled estimate should be removed from the figure + table 2 should be omitted.

We have limited ourselves to the descriptive nature of the study. Indeed with such heterogeneity it wasn’t recommended to report the findings as a meta-analysis. As such, words like pooled estimate have been deleted. We have also re-shaped our conclusions to reflect the fact that it wasn’t possible to statistically assess which scale was more accurate (brief vs long).

In the methods you need to be more specific about the diagnostic criteria that you accepted as ‘depression’. I presume you accepted DSM-IV and ICD-10 criteria but you also need to state which diagnoses were included e.g. was dysthymia included, was it ‘any depressive disorder’, etc. It would also be helpful to include in the presentation of the results.

The diagnosis was that of any depressive disorder. This has been included in the study methods (selection section), the ICD-10 and DSM-IV have also been referenced.

You don’t appear to have given references for the gold standard measures i.e. for CISR, MINI, SCID, etc.

The CISR, MINI and SCID have been referenced.

Page 3 – the EPDS is a 10-item scale (not 15-item) and so should not be considered as ‘long’. The Hanlon reference is not needed (please delete) – we validated using ‘common mental disorders’ as gold standard and so the paper is not relevant to your paper.

This reference has been deleted, and so EPDS no longer appears as a long scale. In the results section, it was moved to the brief scale sections.

Editorial issues (minor essential revisions)
Page 4, para 2 – ‘plugged’ should be ‘plagued’ (I assume?)

Corrected, it’s now spelt as plagued.

Page 7 – add a % to the prevalence figures

Corrected, % added to the figures, now reads 11.1-53.5%.