Reviewer’s report

Title: Psychological wellbeing in a resource-limited work environment: Examining levels and determinants among health workers in rural Malawi.

Version: 0 Date: 22 Jan 2019

Reviewer: Deborah Russell

Reviewer's report:

This manuscript is generally well-written. It investigates factors associated with the psychological wellbeing of mid-level maternal health workers in rural Malawi. The problem this research addresses is of importance as many countries face great difficulties in ensuring a stable and healthy health workforce, especially in rural areas. Understanding the levels of and predictors of psychological wellbeing of health workers is a first step towards remedying high levels of health worker turnover. Indeed, this study highlights the problem of workforce stability when it reports that only 10% of mid-level health workers are in the same job in 2015 as they were in 2013.

Unfortunately, the study appears to have been conceptualized after data collection was planned/undertaken (ie post-hoc), so as the authors acknowledge, not all work-related variables potentially relevant to the wellbeing of health workers were included in the questionnaire and in analysis. Further, the authors' conceptual model does not acknowledge the role of non-work-related factors in influencing wellbeing, nor does the discussion acknowledge the absence of non-work-related factors in the modelling as a limitation. This is important, since the outcome variable measures overall wellbeing, not just work-specific wellbeing. One way to overcome concerns about potentially important variables that may have been omitted from the modelling is to report how much of the variance in wellbeing is explained by the modelling and to demonstrate goodness of fit of the models.

Additionally, while the authors reported the percentage of health workers whose psychological wellbeing was less than 40%, 50% and 60%, their sensitivity analysis didn't extend to examining the results of logistic regression modelling if the different cut-off was used as the dichotomous outcome variable. This analysis could be provided as a supplementary file. Consistent results for the different cut-off levels would provide stronger evidence of the significance of each variable.

Some more minor observations about the manuscript are noted as well.

1. The continuous WHO-5 scores have a skewed distribution. In this instance, reporting median and interquartile range is preferable to mean and standard deviation.

2. Also, consistent reporting of 95% confidence intervals is preferred over p values.
3. I would expect that $\alpha=.05$ be used to test for statistical significance, however this is not clear with reporting of the results. Eg. Lines 12-14 page 14, refers to a statistically non-significant result ($p=.069$), so this should be made clear. Eg. P16 lines 8-10, although this difference was not statistically significant with the continuous outcome variable. Etc.

4. Also, perhaps having received professional training within the last year and having received any supervision within the last month should be consistently categorised as relating to 'organisational support' rather than to 'clinical competence' (eg. Table 3).

5. Table 3, line 20 p30, not sure what 'boni' refers to. Is this a typo?

6. Table 2, line 34 p28. It took me a while to work out what 'PBF exposure' meant. The acronym should be used consistently through the paper.

7. Typo with first reference, should read 'Everybody's'

8. Type p3 line 21 emphatic should be empathic

9. The authors refer to an impact analysis of intervention on PW that is unpublished. Can this be included as a supplementary file (or are there plans to publish separately?). Can further details be provided?

10. Suggest remove subheadings in Discussion section

11. Method for scoring clinical vignettes should be explained

12. One possible explanation for negative association between having received training or actual clinical knowledge and lower wellbeing is that people with high levels of anxiety may be more likely to seek out training and seek out clinical knowledge. Have you tested for associations with scoring on the item of wellbeing that scores anxiety (being calm and relaxed)? This might have implications for initiatives to improve wellbeing by teaching ways to manage anxiety.

13. Another possible explanation of the negative association with training is that the training is poor and this contributes to decreased wellbeing/dissatisfaction, though this is entirely speculative.

14. Basic demographic characteristics measured were limited. Not measured were factors such as ethnicity, marital status, having children, other measure of minority status etc. Statement page 18, lines 11-13 should be amended so that it is clear that while the
research indicated no specific demographic group that could be targeted to improve their PW, there may be demographic factors that weren't measured in this study that are associated with PW or something similar.

15. No explanations about missing data. Did all respondents answer all questions?

16. A little more detail about sampling of participants from the secondary health facilities is needed. How many in total (approximately)? How were they 'randomly' sampled? Were adjustments made for survey sampling methods in analysis?

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