Reviewer’s report

Title: ARE CURRENT CASE-FINDING METHODS UNDER-DIAGNOSING TUBERCULOSIS AMONG WOMEN IN MYANMAR? AN ANALYSIS OF OPERATIONAL DATA FROM YANGON AND THE NATIONWIDE PREVALENCE SURVEY

Version: 0 Date: 23 Nov 2015

Reviewer: Anja Smith

Reviewer's report:

Note: since the pages did not have numbers, I assigned my own page numbers, with the introduction starting on page 1. Please read my comments relative to this numbering system.

This is a valuable and important study in exploring the gender dynamics of TB in Myanmar. It also contributes to a better global understanding and focus on gender and TB. The energy and time invested in compiling an electronic database on health administrative data on TB diagnosis at THDs in Myanmar can prove very important in understanding the health system's ability to accurately and comprehensively detect TB. The impact of this work can be much improved by rethinking the key message/interpretation of the results and addressing some of the following concerns and issues:

1. Title: The current title does not accurately and succinctly do justice to the main message and potential impact of the paper. Yes, the analysis of the operational data indeed reveals a lower case notification rate among women compared to men (which increases for both sexes when a more sensitive testing tool is used). However, the two main issues revealed by the analysis here is how sensitivity of the testing tool may prevent the detection of the true (and potentially higher) prevalence rate of TB among women in Myanmar, as well as the proportionally much larger group of women diagnosed with TB in the age group 18-25. By phrasing the title in terms of a conclusion (something like "Current TB testing tool may under-capture TB among women in Myanmar") you are likely to have much more impact, relevance and will potentially enlarge your readership.
2. Abstract:

a) It may be useful to indicate the magnitude of the increase rather than simply state "could result in a higher proportion of females" (lines 41-45).

b) This particular analysis has not convinced me that socio-demographic factors influence sex differences in tuberculosis case finding (lines 49-51). Yes, there is a substantially higher proportion of women in the age category 18-25 diagnosed with TB compared to their older counterparts which could point towards socio-demographic factors, but this warrants further exploration before attributing causality, even in cautious language. If you want to hint at the possibility of socio-demographic factors driving this difference, greater discussion and unpacking of this (in the discussion section) is required. I provide further comments on the issue below.

3. Please rephrase "discrepancies in physical access" (p.2, line 10) as "discrepancies in access" and indicate that this is about more than physical access barriers. Access to healthcare is a multi-dimensional concept which includes components such as physical access barriers, financial access barriers and other (also cultural) access barriers (see, for example, Penchansky and Thomas, 1981). The cited literature (references 21-26) not only includes discussion of physical access barriers but also other important factors such as the differential nature/impact of stigma between men and women and gender roles that may impact health seeking.

4. Another potential explanation for gender differences in TB being correctly diagnosed at health centres (p.2, lines 12-16) is possible differences in the TB symptoms with which men and women present at health centres. See the following references in this regard:


5. I would suggest rephrasing the summary/conclusion about factors driving gender differences in global TB notification rates in more tentative language. You could consider using the word inserting the word "potentially" in the first part of the sentence ("The first two explanations point towards a potentially true difference…") and the word likely in the second part of the sentence "whereas the second two support the notion that the observed sex difference is likely due to more missed diagnoses of TB in women" (p2, lines 20-25).

6. What is the source/reference for the following statement: "In South East Asia, only 40% of TB diagnoses are from women." (p.2, line 28). Perhaps make it clearer if the source for this sentence/statement is reference 11, i.e. insert 11 at the end of the sentence.

7. I wonder if you shouldn't rather make the point that "operational data has never been collated" (p.2, line 36). This strengthens the contribution of your study.

8. Results section: In the text (p. 5, line14) you indicate a CI of 28-33 for 30% of women diagnosed at THDs. However, in Table 1 the CI is indicated as 28-32. Please double check and report the correct one.

9. Results section: In Table 2 the first age category is reported as 18-24. In the text you, however, refer to 18 and 25 (p.5, line 32). Please correct in the text.

10. Results section: Please include a reference to Table 2 in the last paragraph.

11. Results and national prevalence survey: In terms of the analysis of the national prevalence survey data, is this a sub-sample of the larger national prevalence survey? The sample sizes reported in Table 2 seem low. Please indicate the sample size of the national prevalence survey and how the data reported here (if a sub-sample) relates to the larger national prevalence survey. You could do so in both the data analysis discussion (p. 4) and in the caption to or in Table 2 itself. It is perhaps a good idea to say something about the sampling strategy of the national prevalence survey. I assume the survey was designed to be nationally representative in terms of gender, ages and geography. Could the prevalence survey itself have had a gender bias because of sampling at specific times of day or at specific places?
12. Results/data analysis approach: While it is useful to report the confidence intervals for the mean proportion of women in terms of the different indicators, your analysis could be significantly strengthened by also including a column in Table 1 to indicate the significance levels of mean/value differences (for each line/indicator) between men and women (i.e. the gender difference). This can easily be done through use of the chi-square test ($\chi^2$) and will help you to identify other important patterns which deserve consideration. This would, of course, imply reporting the main results in this regard in the text and interpreting it in the discussion section.

13. Results: Should the heading of the second grouped column in Table 2 not rather have read as follows: Prevalence survey (culture positive when microscopy negative)?

14. Discussion section: You indicate that stigma "particularly affects care-seeking decisions of young women who are close to marriageable age or recently married" (p6., lines 47-51). The large proportion of women in the age group 18-24 (i.e. potentially close to marriageable age) is, however, contrary to this statement (I, however, acknowledge that the decrease in the proportion of women in older age categories lends support to inclusion of this statement). Your discussion of age requires further interpretation. There are at least two other factors to consider in relation to the proportionally much higher diagnosis of TB amongst women aged 18-25 compared to women in the other age groups. In the introduction you mention that Myanmar is experiencing one of the most severe HIV/AIDS epidemics in Asia. Is it possible that HIV infection rates among younger women are higher than their older counterparts and that this could help to explain part of the higher TB prevalence rate? In South Africa, young women bear the brunt of the HIV burden due to complexities around sexual relationships and age-discordant relationships. There are thus high rates of HIV and tuberculosis co-infection among younger women which may also be the case in Myanmar.

A second explanation could be around household and childcare duties preventing women from getting to the health centre (this may be less of a constraint for very young and older women). It would be important to achieve greater depth in this part of the discussion if you want to conclude that socio-demographic factors may indeed influence sex differences in TB case finding. See the following references in this regard:


If word limit is a problem, I would suggest slightly decreasing the proportion of the discussion devoted to the sensitivity of the current testing approach.

15. Discussion section: In the results section (p.5, lines 19-23) you mention evidence of variation by month of diagnosis for female patients. Can you venture a potential explanation for this finding in the discussion section? Could it be related to the seasons and agricultural activities (also women's roles in these activities)?

16. Discussion section: A more nuanced potential explanation for the variation in the proportion of females comprising total diagnoses at different THDs (relates to p.6, lines 18-22) is variation in health worker testing biases or health worker adherence to testing protocols (also an indicator of health care quality) across sites. At least two studies have now found that women who present with the same symptoms as men at health centres/clinics are significantly less likely to be tested for TB and these studies speculate that health workers may have strong stereotypes/beliefs on the typical TB patient being male rather than female. I include reference to both studies:


17. Discussion section: From a language and style perspective, the last two paragraphs in the discussion section are quite long. I suggest breaking the second last paragraph into two paragraphs and starting your new paragraph at "While use of culture or other more sensitive..." (p. 7, lines 29). For the last paragraph I would suggest breaking it into two at "It is also important to consider that our study..." (p. 8, line 21).
18. Discussion section: Furthermore, I suggest replacing the word "great" (p. 7, line 30) with "substantial".

19. Discussion section: In keeping with the same type of comment above, I suggest removing the word "very" from p.7, line 40.

20. Discussion section: The point in your last sentence of the paper (p.8, lines 28-36) should not be about the sensitivity of TB diagnosis at THDs that "may differentially affect case finding in women", but rather that the sensitivity of TB diagnosis at THDs may lead to a potential under-diagnosis (or under-capturing of cases) of TB among women. It would be good to indicate the direction of the differential impact on case finding for women.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.
Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.
Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.
No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.
I am able to assess the statistics

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