Author’s response to reviews

Title: Myanmar mortality registration: an assessment for system improvement

Authors:

Myitzu Tin Oung (myitzu@gmail.com)

Kerry Richter (krichter99@gmail.com)

Pramote Prasartkul (pramote.pra@mahidol.ac.th)

Viroj Tangcharoensathien (virojm16@hotmail.com; viroj@ihpp.thaigov.net)

Version: 2 Date: 17 Apr 2017

Author’s response to reviews:

Response to Reviewer reports:

1. This is an interesting and important addition to a very sparse literature about the mortality registration and cause of death data system in Myanmar. However, the scientific platform for trying to meet the stated aims of the paper (evaluate the quality of mortality data collected by the vital registration system) are not well explained.

The authors should start by explaining how the VR system maintained by the CSO in Myanmar functions. In 2014, 200,000 deaths were registered. How? What happens when someone dies in a village (85% of deaths in Myanmar)? How are deaths reported, by whom, to whom, and how are they consolidated at CSO in the capital, and how is the cause of death determined, by whom. What is their opinion about the likely impact of these current procedures on completeness and cause of death data quality? What fraction of the 200,000 deaths occurs in hospital? Do doctors in hospitals know how to correctly certify the underlying COD? What evidence is there to support their claim about diagnostic accuracy of hospital deaths?

The authors added the suggested information in “Introduction” section; from 3rd to 7th paragraph.
2. Next, they should explain and justify the data quality evaluation framework they choose to use. It is NOT a University of Queensland framework; it is from a Working Paper by Abouzahr, Mikkelsen and Lopez who were all at the University of Queensland when it was published. Needs proper attribution. What do they think of this framework? Why select it over others (Rao et al; Mahapatra et al)? Once justified, they should explain that the framework consists of a series of steps derived from decades of observations about demographic and epidemiological trends in countries where data are more reliable. Is that appropriate to evaluate data quality in Myanmar? Make a compelling case, don’t just apply the framework in an uncritical and unjustified way.

3. Briefly introduce the 10 steps in the Abouzahr et al paper, and explain which ones are being applied in Myanmar in this paper, and why?

The authors added two paragraphs (1st and 2nd paragraph) under “Assessment methods” according to reviewer’s suggestions no. 2 & 3.

4. Pg 7 of revised paper: what is the evidence that the age/sex reporting in the 2014 census was “fairly accurate”?

I provided the following explanation for why I said “fairly accurate” on page 9; under “Methods” section; “Preparation for the assessment of the quality of mortality data from the VRS”

“Before applying the method, the quality of age and sex structure of population from the census was assessed by constructing the age-sex pyramid of the population and calculating Whipple’s index, Myer’s index, age and sex ratios and joint score index.

The age-sex pyramid of the population shows minor age misreporting, especially age ended in 0 and 5. The Whipple’s index was 123 (i.e. quality of data is acceptable as the deviation from perfect was less than 25%). The Myer’s Blended index was 9.7 for male and 9.9 for females, indicating minor age misreporting, with preference for ages ending in 0 and 5. The age ratios of both male and female were close to 100 across age groups from 15 to 64 years. The sex ratios pattern followed the typical one (i.e. the sex ratio is slightly over 100 at the early ages due to more male than female births and is reduced continuously up to the oldest ages since mortality is usually higher for males than females.). The joint score (JS) index is 13 (JS < 20: accurate of age and sex structure). The findings suggest the age and sex structure of the census population was fairly accurate.”
If the reviewer suggested to add figures and tables that show the above assessment, we can provided them in the Appendix.

5. Pg 8 and other places: why do they not use the extensive GBD studies to assess reliability of the Myanmar data, in addition to, or instead of, the UN, US Census Bureau etc. The GBD is much more comprehensive, published annually in the Lancet, has over 2000 collaborators worldwide, compared to a handful in the UN or US CB. If they chose to ignore it, they need to justify why.

I added the results obtained by using number of deaths and COD information from the GBD study under all relevant sections: the Crude death rate, Distribution of major COD, Age pattern of broad groups of COD, Leading COD, Ratio of NCD to CD in the COD and Assessment of completeness of death registration.

5. Table 2 showing the top 10 causes of death in 2013 in Myanmar: how was the cause of death data generated? Why should we believe them? See comment #1.

Ok. I have explained the death registration practice in Myanmar in “Introduction” section. Majority of COD information are noted by midwives, who have not received a proper training to certify COD, based on the information provided by the family members. Thus, even though the COD are included and defined with a proper code, their reliability is not sure.

The purpose of describing Table 2 is not to inform the top 10 COD information in Myanmar, but just to verify that the COD generated from the VRS are not reliable by comparing with other source of COD information. I added the comments on the finding: “The finding suggests that there are some problems in certification of cause-of-death or coding practices.”

6. Table 3 contains a lot of useful information by comparing the VRS data with the 2014 census reports. We need to know a little more about the reliability of the census reports. For example, did the census really record 485,000 (which would correspond to a CDR of 9.7 reported in Table 3) or is this an adjusted number? If adjusted, on what basis? Same for child deaths. Are these census levels of IMR and 5q0 actually reported or adjusted levels based on what was reported,
and if so, how? Convince the reader that your standard for comparison (the 2014 census) is believable.

The mortality indicators in Table 3 derived from the census are all adjusted ones using the Brass method. The information about how to derive such indicators in the Census are described in “Methods” section; under “Assessment methods”; 5th para.

7. Page 18, just before Conclusions and Recommendations: add a reference to the dual –records method. It is a method, not a system.


In the reference, it was mentioned that “A dual-records system is a system of demographic data collection that uses two independent types of data-collection activity to record the same events occurring in a certain period of time and in a defined area …..” That’s why I used that word. But if you prefer, it can be changed into “dual-records method”

8. Same page, para 1 of Connclusions: what is the source for saying 25% of deaths are certified by a MD? I thought only 15% died in hospitals. Where do the other 10% of certified deaths come from?

In the first paragraph of “Results” section, it was described that “In the 2013 VRS, there are a total of 199,491 registered deaths, of which 44,454 (22.3%) are ill-defined COD. Among all deaths, 82.9% took place at home and 24.2% were certified by medical doctors.”

To clarify this, I would like to show you the following table. That information was obtained from performing descriptive analysis of 2013 VRS raw data provided by the CSO, Myanmar. The hospital deaths are usually certify COD by registered doctor in attendance. However, some deaths occurred outside hospitals or even some deaths occurred at hospitals but brought back home without registering deaths at hospitals are certified by doctors not in attendance.

Persons certifying cause-of-death

<table>
<thead>
<tr>
<th>Persons certifying cause-of-death</th>
<th>Number</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Registered doctor in attendance</td>
<td>33,784</td>
<td>16.9</td>
</tr>
<tr>
<td>Medical registrar</td>
<td>148,672</td>
<td>74.5</td>
</tr>
</tbody>
</table>
Other registered doctor not in attendance 14,556 7.3

Other health personnel (PHS I or II, LHV, MW, AMW) 2,479 1.2

PHS: Public Health Staff; LHV: Lady Health Visitor; MW: Midwives; AMW: Auxiliary Midwives

9. Last para: the authors should mention the extensive effort to improve the VR system in Myanmar that has been going on since 2015 under the Bloomberg Data for Health Initiative. The MoH (Thet Thet Mu, Aye Aye Sein) and the CSO (Director Wau Wau Muang) are very familiar with it. Say something about what it is doing and what it is likely to achieve to improve the system.

Thank you. I have added the information obtained from personal communication and from available website in Conclusion and Recommendation section, 3rd and 4th paragraph.

Reviewer 1

• I would like to thank the authors for the revision of their paper, and to address most of our comments and concerns.

Thank you very much for your comments and suggestions for the improvement of this manuscript.

• The authors have addressed parts of my concerns about their application of the Brass Growth Balance (BGB) and the presentation of their results.

They mention in their reply that "Comparison with deaths from the census (at national and subnational level) was done in the revised version of the manuscript, with both raw data and adjusted mortality indicators." But I only see the adjusted mortality indicators, not the comparison with the raw death counts as I requested in my point #5 which the authors have dismissed. (#5. Page 13 end of para 1: add a table and scatter plot comparing death counts from VRS and from census overall, by sex and subnational units.) I checked the journal guidelines and practices, and there are options for additional tables in online appendix (FYI https://pophealthmetrics.biomedcentral.com/articles/10.1186/s1296301701228).

I respectfully disagree with the response of the authors that the comparison between two data sources has already been made with reported deaths (unadjusted) and adjusted mortality estimates, at least in its current form.
In the previous revision, I stated that I did comparison with unadjusted data from census, in which I mean I used ASMR, Age distribution of deaths from census, which are calculated from unadjusted death counts. But I did not perform with unadjusted death counts from the census.

In this revision, I added a scatter plot as suggested by the reviewer “Figure 5, Appendix A” to display the discrepancy between death counts from two sources by region and gender, page 15 under “Assessment of completeness of 2013 death registration in the VRS.”

• If they refer to their table 3, then this paper needs substantial improvements in the presentation of these unadjusted and adjusted information and its related comparisons. This part from my point of view is an essential aspect and contribution of this work and deserves more clarity. page 12 lines 34+ Death Distribution Methods: Brass Growth Balance method and Preston and Coale method OK. But Table 3 shows Completeness of death registration (%) that are different from any of these 2 methods. Either the results in the text are wrong or the table is wrong. Correct and/or label properly the column in the table to understand what method was used to get the numbers in Table 3.

Assessment of completeness mentioned in Table 3 belongs to the first method, using estimated number of deaths from other source like the 2014 census. BGB and Preston and Coale are second method “Death Distribution Method”.

To avoid confusion, the structure was re-arranged. Table 3 and relevant texts were moved above, the second paragraph. Only after finishing the first method, the results from BGB and Preston and Coale was described.

• Are the results in Table 3 based on the first assessment method described in lines 3640 on page 6? If so this needs to be made much clearer on pages related to Table 3. Currently the text related to Table 3 would suggest that it is the ratio of 2013 VRS columns in table 3 by those of the 2014 census (report) which themselves are based on ”adjusted data from the report” (line 49 page 12). This explanation is extremely confusing, and the reader has no idea what kind of adjustments or methods were used to derive these adjusted 2014 census mortality indicators used as benchmark. It is for instance unclear how the infant and child deaths and related rates from the census have been obtained and why kind of adjustments were done: i.e., based on recent household deaths and/or indirect estimation from children ever born and still living?

The mortality indicators in Table 3 derived from the census are adjusted ones using the Brass method. The information about how to derive mortality indicators in the Census are described in “Methods” section; under “Assessment methods”; 5th paragraph.
Reviewer 2

- The manuscript has been improved significantly by inclusion of estimates of completeness of registration using both UN and census estimates.

Thanks for your comment.

- However, the use of the census estimates also needs to be complemented by a description of the method used to produce adjusted census death data (i.e. Brass Growth Balance method), and reiteration of its drawbacks.

It was described on page 8; paragraph 5; under “Assessment methods” of revised manuscript and page 21; paragraph 13; under “Discussion”.

- Further, while there is clearly scarce information with which to estimate completeness at the subnational level in Myanmar, and comparison with the adjusted Census crude death rate is one method with which to make such an estimate, the limitations of the Census adjustment method at the subnational level (i.e. using Brass Growth Balance) should be described with the results. While it is clear that these estimates are not intended by the authors to be exact but more a guide to the approximate level of completeness, such a description of the methods and limitations at the subnational level is necessary.

It was described on page 8; paragraph 5; under “Assessment methods” of revised manuscript and page 21; paragraph 13; under “Discussion”.

- The assessment of completeness should also report the Global Burden of Disease estimate of total deaths and 95% uncertainty intervals, to demonstrate the uncertainty of the estimate of total deaths in Myanmar given the lack of data.

Thanks for your suggestion. I added the results under “Assessment of completeness of 2013 death registration in the VRS” as “The completeness of death registration was 48.6% (34.7%-70.5%) if it was calculated based on the estimated number of deaths from the GBD 2015”

- In terms of the structure of the paper, it would make sense to move the completeness of registration section to where the crude death rates and child mortality rates are reported.
The structure of the paper I am trying to display in both methodology and result section is:

First - describe the assessment of quality of mortality data using the guide by AbouZahr et al. 2010;

Second – describe the assessment of completeness of death registration using two methods; (1) by using estimated number of deaths from other source and (2) by using Death Distribution methods

I hope you agree with this arrangement.

Reviewer 3

The authors have revised the article to my satisfaction.

Thank you very much for accepting the re