Author’s response to reviews

Title: Estimating Adult Mortality in Papua New Guinea

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REVIEWER’S COMMENTS – POPULATION HEALTH METRICS December 2018

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Estimating Adult Mortality in Papua New Guinea
Urarang Kitur; Tim Adair, PhD; Alan Lopez, PhD
Population Health Metrics

Reviewer reports:

Reviewer #1: This is a well prepared article of particular interested to anyone interested in the demography of PNG and, more generally, in the increasing understanding of methodological and data issues in measuring adult mortality and the need to look carefully at country-specific circumstances and not to place an over-reliance on estimates that make use of standardized methodologies across a range of regions and countries. The fact that it provides the first provincial estimates of adult mortality as well as a socioeconomic index for help in assessing veracity is also of use for people working in and on PNG.

We thank the reviewer for their helpful comments.

I have only a few substantive comments:

(1) First is to note that the only "new" addition to methodology here is the method for adjusting orphanhood-based estimates from a single census for under-reporting. While this is described in the text this is only in narrative terms. As this method happens to be a product of two of the authors and is only coming out in a forth-coming paper, it might be useful to include a brief Annex outlining the math (formulas) involved in the calculations.
The paper referred to has been published since this manuscript was originally submitted to this journal. The reference (23) has been updated. Full details of the equation used is found at: https://journals.plos.org/plosone/article?id=10.1371/journal.pone.0197047 Adair T and Lopez AD. Estimating completeness of death registration: an empirical method. PlosONE. 2018; 13(5): e0197047. doi: 10.1371/journal.pone.0197047

(2) Second, on the composite index it is not entirely clear what is the meaning of "it is adjusted to be a normally distributed percentage." A bit more explanation might be useful that could likely be done in an added footnote.

We have added a footnote at Line 234 and amended the sentence to better describe the procedure employed. Each value was adjusted to a normally distributed percentage ranging from 0 to 100% calculated using the mean and standard deviation of all values in that particular indicator.

(3) Finally, in reading the article I spotted a very few seeming grammatical errors or inconsistencies. A final proofreading before publication might be useful but it is till virtually 100 percent of the way there.

We have reviewed the paper and amended the various grammatical errors.

Reviewer #2: Thank you for the opportunity to review this manuscript. It is excellently written and provides extremely important developments to this area of work in Papua New Guinea. I have some mostly minor comments and suggestions.

We thank the reviewer for their insightful comments.

1) In the Methods section the authors provide a comprehensive description of the possible data sources and their strengths/limitations. It would help the reader, I believe, if this information was tabulated.

We have added this information to new Table 1.

2) In most of the text the authors refer to Simbu province but the maps use the spelling Chimbu. The maps are very difficult to read and the embedded text should be made clearer.

We have added a note to the maps that Chimbu refers to Simbu.

3) The sentence about the 2006 census between 369 and 371 can be deleted as it seems redundant given the previous section.
We have deleted this sentence.

4) I am most concerned about the Composite Index used in this analysis:

4a) First please provide the years for which the education indicator is based (I note the publication is from 2010 but when was the data collected)

The education data were collected from 2007 to 2009 and were based on PNG Department of Education statistics. We have amended the sentence in Line 235 to specify that it refers to the average of net enrolment rate (proportion of 6 year old who are actually enrolled in Elementary prep and those who are not) and female adult literacy rate. The net enrolment rate is a good indicator of access to Universal Basic Education attainment while female education has been linked to decrease child mortality.

4b) please provide more details on the economic indicator and how the World Bank assessed 'poverty levels'

The economic indicator is an average of poverty levels as assessed by the World Bank and the proportion of people engaged in paid work activities from the 2011 census.

The World Bank report uses a poverty line is used which allows for food expenditure for 2200 calories per adult equivalent per day and an allowance for basic non-food expenditure. Their analysis found similar trends to if they had used a simple international measure of $1/day.

4c) the use of 'the proportion of people engaged in paid work' is the most problematic of all the indicators here. It explains why NCD achieves such a high score but brings up a number of issues. Essentially any urban area will score highly on this indicator while those living off of land will have a low score. The relationship of paid employment with health in the PNG context is likely to be reasonably complex. This needs to be unpacked here. It would also be good to see sensitivity analyses and/or a discussion of how the composite behaves in comparison to others' results.

The proportion of people engaged in paid work allows for a more comprehensive measure of economic status than just using the World Bank poverty level, because income from paid work significantly increases the ability of people to access health care and resources that reduce risk of mortality and morbidity. We recognise that this is a complex area of study, hence we combined the poverty and paid work indicators. There are no other similar analyses that have been conducted in PNG that we are aware of.

5) The authors mention where their findings are in agreement with Bauze's analysis. A more engaged discussion of the comparison between these analyses would be welcomed.
We agree that this is a worthy area of further analysis. A separate manuscript from this study that explores life expectancy and mortality at all ages in PNG analyses the provincial-level relationship between adult and child mortality more closely. Therefore, we did not include this analyse in this manuscript. In summary, the relationship between child and adult mortality was stronger for males (R²=0.651) than females (R²=0.3766). For males, there were no provinces with clearly disparate 5q0 and 45q15 estimates. However, for females, Simbu and Central have relatively low 5q0 and relatively high 45q15. We have added a sentence at Line 337.

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Dear Dr. Kitur,
I've just sent a revision request for your paper, and wanted to also include comments from the editor. You may choose to respond to these if you wish; if these comments have already been addressed, please feel free to note this with no obligation to provide an answer.

1. Data availability from 2001 and 2011 censuses is somewhat different for males and females, as males were not asked the parental survival questions in 2001, though the implications of this is not discussed at length.

We thank the editor for these additional comments. We would like to clarify regarding data availability. Males were asked the parental survival question in both censuses; the issue is that a question of the parental survival of males was not asked in 2000 census. We developed our methodology accordingly; for males, we applied the orphanhood method to estimate adult mortality form the 2011 census, and then adjusted this using the Adair-Lopez completeness method. For women, we used the hypothetical cohort orphanhood method from the 2000 and 2011 censuses. We then forward projected the two sets of estimates to a reference year of 2011.

2. The national estimates in this paper are consistent with UN estimates based on parental survival from 2000, but little is said about how the UN estimates were arrived at. The UN estimates were derived using parental survival data from the 2000 census to estimate adult mortality (45q15), and detailed child mortality data from the 1996 and 2006 demographic and health survey and summary birth history data from the 2000 census to estimate child mortality. These estimates of adult and child mortality were then input into the Far Eastern model life table from the UN model life tables. We have amended this sentence in Line 354.

3. The national estimates are far lower than GBD estimates, and some explanation is offered regarding HIV adjustments and patterns of NCDs in nearby countries, but there could usefully be more discussion.

We have clarified that the model life tables of the GBD are how the other Pacific countries’ mortality impacts the estimates for PNG. HIV prevalence in PNG is quite low; antenatal surveillance among 15-24 year olds provides an indication of the proportion of the sexually active population that are infected with HIV. The HIV prevalence amongst this group in PNG has been under 1% since 2011. This supports us not making any adjustments for HIV. We have added a reference of this data (Line 357).

4. All indirect methods for mortality estimation, such as orphanhood, tend to assume fairly stable mortality over time, but there is no discussion of defence of the use of this method here.

The hypothetical cohort orphanhood technique is utilised to estimate intercensal female adult mortality – therefore this approach does not make an assumption about stability of mortality over time. For male mortality, which is derived using one census, the orphanhood method allows for adult mortality to be estimated for a reference date in the past and does not assume constant mortality levels. Analysis of the adult mortality estimates for each 5 year age group and their respective reference dates show that there is indeed a steady decline in mortality over time.
5. Orphanhood estimates also have a cultural component in traditional societies such as PNG, where orphaned children may be "adopted" by other family members, and thus not considered orphans, thereby resulting in underestimation of mortality using this approach. There is no discussion of local practices in this regard.

The potential for under-reporting due to the relatively common practice of adoption in PNG is now acknowledged in Lines 192-194. The census questions ask whether the person’s “own father” or “own mother” is still alive, which can enable biological and adopted parents to be distinguished. We have assessed completeness of reporting for males using the Adair-Lopez method and adjusted estimates accordingly, which would overcome any issues with under-reporting of mortality because of this issue. For females, estimates of adult mortality were made using the hypothetical cohort orphanhood technique which uses two sets of parental survival data (2000 and 2011) and so any under-reporting would likely be consistent between the two sources and therefore not affect estimates.

6. These methods assume reasonable quality of age reporting, but there are reasons to suspect that such reporting is of poor quality in this society. Some discussion would be useful on this.

The impact of age misreporting in the data on mortality estimation should be minimal because all data are analysed in 5 year age groups and respondents are aged under 45 years, where misreporting is typically much lower than at older ages.

7. PNG has had a long term demographic surveillance over decades at Tari, run by Prof. Ian Riley in the early days. It has also had a long running (DSS) at Wosera run initially by Ivo Mueller and his colleague, Su ?? (from Thailand). No reference is made to the consistency of these provincial mortality estimates to these high quality sources of mortality data.

A comparative analysis conducted on 8489 deaths in Tari, Asaro, Wosera and Madang between 1970 and 1995 by Goude and colleagues reported 80% of deaths from Tari due to enrolment of larger sample size over longer periods of time. These data sources did not calculate adult mortality (45q15) so it was not possible to make direct comparison of our findings with these studies. These studies become more relevant comparisons with analyses in our subsequent papers on all-cause mortality and cause of death.

Best,

Pauline