Author's response to reviews

Title: Differential mortality in Iran

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Author's response to reviews: see over
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Editorial Team
Population Health Metrics

Re: “Differential Mortality in Iran”
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Thank you for your email of 9 June 2007 inviting us to revise our manuscript on the basis of reviews received and to resubmit to PHM. We have addressed the comments of the editorial team and reviewers as follows:

Reviewer: Farid Abolhassani

Page (6): “The beginning year of Mortality data collection by Deputy of Research and Technology seems to be later than 1965…”

According to two references (14 and 15 in the manuscript) MOH&ME (Deputy of Research and Technology) collected data on causes of death for the first time in six cities of Iran (Tehran, Mashhad, Esfahan, Shiraz, Kerman, and Orumie) in 1965 and published them. The second data collection was implemented in 1966 in 10 cities. The third and the fourth series of data collection were carried out in 1970 and 1972, respectively. Over the period 1979-1995, this data collection was enlarged to include 24 cities. Finally, for the period 1996-2001, data were collected under this system for entire country. Hence, the year 1965 for the beginning of this death registration system is correct. (The references cited are from the revised manuscript).

Page (9): The last sentence of the page “that is death reported in the independent survey of mortality...” is not clear and needs to be rewritten.

The sentence has been revised as requested. It now reads (see the last paragraph on page 9):
“That is, deaths reported in an independent survey of mortality are compared to deaths reported in the death registration system for the same population, from which unmatched and unrecorded deaths can be identified and estimated.”
Page (11): Second paragraph, first sentence: “adult mortality is modelled as function of either GDP/capita or literacy”. Sentence to be revised to reflect this.

This sentence has been revised on page 12, last paragraph (first three lines) as follows:

“Next, we modelled adult mortality level for provinces from groups (a) and (b) above as a function of the two socioeconomic indices (GDP/capita and % literacy) using linear regression.”

Reviewer: Emi Suzuki

The number for the figures is missing.

According to the instructions for preparing the manuscript the figure number should not be included with the figures. We presume that the editors will decide on this as a matter of style.

Table 1, 2, 4 have confusing alignment.

This has now been corrected in the revised manuscript.

There are some typos (page 3, 9, 10, 15).

We thank the reviewer. These and a number of other grammatical errors in the text have been corrected.

Response to Editorial Team

The PHM editors discussed the paper and had some concerns about the apparent subjectivity of the decisions whether or not to use the Brass method province-by-province. The Brass method "worked" apparently when completeness was high; it did not work in most other provinces (most labelled 'c' in table 3 with only 2 labelled 'b', i.e. plausible).

The editors would like to ask you to provide a more explicit description of the criteria used to decide when the Brass method was plausible/useable, and also to address in the discussion the following two issues.

We have clarified the criteria used to decide when the Brass method worked and when it did not work. These are not subjective criteria, but are based on actual values of the correction factor (K) in the method. When the slope of the fitted line between partial birth and death rates (K) is less than 0.9, the implied level of completeness (1/k) exceeds 110%. We took 110% as the upper bound of
completeness that, in reality, was actually 100%. This minor exaggeration of completeness can be attributed to violations of assumptions of population stability and/or age misreporting. Similarly, Preston has recommended that the Brass method not be used to assess completeness when the implied level of completeness is less than about 60% (see reference [30] in the revised manuscript). There is also a commonsense logic to a lower bound around this level. If estimated completeness is below about 50-60% then there must be serious concerns about the quality of the data. This corresponds to a value of \( k \) of about 1.65, or to be stricter, we have rounded this down to 1.5. This was the reasoning behind the criteria for applicability of the method. We have added a sentence (lines 4-6) on page 11 of the revised draft to explain this.

To provide additional perspectives on the estimation process, and the various approaches we have used to assess plausibility of the implied levels of adult mortality, we have moved some of the paragraphs from later sections of the paper to this section of the methods. Hopefully, this will provide a more complete and informative perspective on the criteria and methods we have used to estimate adult mortality, including the Brass Growth Balance approach. These paragraphs have been added (moved) to page 11 and 12 of revised draft.

The general issue of the shortcomings or problems with the Brass method.
We agree that this requires additional comment and have added a paragraph to the Discussion on page 20 as follows:

“The method has several limitations. First, it might be that the assumption of a linear relationship between the partial birth rate and the partial death rate is violated. This could be due to several reasons such as misreporting of age, particularly at advanced ages [34]. When these points are excluded, different estimates of completeness will be obtained. A second source of bias for this method is that the completeness of death registration might vary by age which cannot be assessed from this method [30]. Perhaps most importantly, the method is sensitive to violation of the assumption that the population is stable and closed to migration. This is very rarely the case [30]. In addition, the method is highly sensitive to the estimated slope of the regression line of partial birth and death rates, which in turn defends on the statistical rigour of the method used to estimate the registration.”

Why few results of the Brass method that generated higher incompleteness estimates were deemed plausible.
We have added a paragraph to the Discussion on pages 20-21 to respond to this query as follows:
“Where the method yielded estimates of incompleteness that were relatively high (60-80%), we have accepted them as plausible based on other comparative data and information about the socioeconomic development of provinces.”

We trust that we have adequately addressed the reviewers and editorial committee’s comments, and we look forward to hearing from you.

With best wishes.

Yours sincerely,

[Signature]

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