Author's response to reviews

Title: Patterns of mortality in public and private hospitals of Addis Ababa, Ethiopia

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Author's response to reviews: see over
Response to a reviewer comment (Andre Kengne)

Title: Patterns of mortality in public and private hospitals of Addis Ababa, Ethiopia

Authors would like to thank the reviewer for his constructive and valuable comments and recommendations for the improvement of this article. As he rightly put it, this article will contribute a lot to Ethiopia and the region at large regarding the double burden of communicable and non-communicable diseases. We have given the comments one by one here under with and incorporated in the text.

Major Compulsory Revisions

1. Causes of death vary by age and likely across public and private health facility. It will improve the presentation if this structure was used throughout the entire result section (i.e. always presenting the age-specific data by public and private facilities, before presenting the overall summary).

Response 1: We have accepted this comment but 88% of the deaths were from 12 public hospitals and only 12% of the deaths were from 31 private hospitals. This could be less relevant to made comparison of public and private hospitals.

2. The data presented in the study was collected over a period of 8 years or so and is sufficiently large. It will be helpful to assess the time-trend in the data presented. That’s is the authors should consider slicing the data for instance into 4 time period of 2 years each for instance and see if the pattern remain the same over time. It is of note that the data collection spans across the period of improved access to HAART in Africa, and time-trend may reveal effect if any on mortality in this country.

Response 2: We have accepted this comment and tried to see the time - trend of mortality but we couldn’t get relevant findings.
3. This study, just like any investigation has got some limitations. The authors touch in this briefly in the first paragraph of the discussion. But a dedicated and elaborated section on the strengths of limitations of the study is needed. For instance if verbal autopsy has been used alongside doctor-diagnosed cause of death, the possible implication has to be discussed.

**Response 3: We have accepted this comment and included more limitations and strengths of this study. Verbal autopsy and hospital surveillance are independent surveillance projects in our program and obtained IRB clearance for the program. We have already used hospital data to validate verbal autopsy data and published with BMC public Health. We haven’t been used verbal autopsy along side doctor-diagnosed cause of death. We made more clarification in the ethical clearance.**

4. Methods: Paragraph 1: Reference 14 relates to a paper only submitted. It should be removed and more description of the study setting provided.

**Response 5: We have accepted this comment and removed the reference and discussed briefly.**

5. Methods: Data collection procedures: In such a study using patient’ files and registries, one would think that the investigators extracted causes of deaths as provided in the patients file/registries, and therefore had no access to the causes of death certification process. If so, the statement relating to whom and how physicians reached the cause of death would be inappropriate and should be removed. If the conducted VA studies for all deceased as indicated in the Ethical clearance section, then this should be clearly stated as so. In the even that VA was conducted only for part of the deaths, then sensitivity analyses should be conducted excluding those deaths, to confirm that results are still similar, or report any change.
Response 5: We have accepted this comment and removed the statement. Sorry for the confusion that hospital data and verbal autopsy data are independent data sources. We haven’t been used verbal autopsy along side doctor-diagnosed cause of death. We have removed verbal autopsy related statement from the ethical clearance section (see also response3).

6. Methods: Data management and analysis: Please indicate the variables adjusted for in regression analyses. Odd ratio is the effect size, not a measure of the strength of associations. The reference to OR as a measure of strength should be removed.

Response6: We have accepted this comment and removed the statement “OR as a measure of strength of associations”. We didn’t have variables for adjustment in the regression analysis and it is crude OR.

7. Ethical clearance: It would be important to indicate the protocol used for verbal autopsy as to confirm the appropriateness in capturing all the causes of death of interest in the current study. Similarly, it will be important to describe how verbal autopsy based causes of death were adjudicated.

Response7: Please see response 3 & 5.

Minor Essential Revisions

1. Abstract: The two last sentences of the result are just repetition and should be removed.

Response1: We have accepted this comment and removed the sentences.

2. Introduction: Paragraph 1: the whole explanation of the ‘epidemiological transition’ concept could be removed.

Response2: We have accepted this comment and removed.
3. Introduction: Paragraph 2: the authors should consider a more recent alternative to reference [8]

**Response3:** We have accepted this comment and changed the reference.

4. Introduction: Paragraph 2: the last sentence does not capture the full spectrum of actions needed to reduced mortality in the context of double-burden of diseases, which should cover both ‘control’ and ‘prevention’ of diseases

**Response4:** We have accepted this comment and modified the statement.

5. Results: Table 2: 2 decimal point for the OR and 95%CI should be enough; 1 should be used as odd ration for the referent category and N/A (not applicable) used to replace ref. in the significance column.

**Response5:** We have accepted this comment and included.
Response to a reviewer comment (Zunaid Karar)

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Authors would like to thank the reviewer for his constructive and valuable comments for the improvement of this article. We have given the comments one by one here under with and incorporated in the text.

Minor Essential Revisions

1. Under Study Settings subsection, both the references provided on explanation of study setting are yet to be published – please explain in this manuscript as well.

Response 1: We have accepted this comment and removed the reference and discussed briefly.

2. Except the first sentence of the Study Design subsection, the rest of the texts should be moved to Study Settings subsection. This subsection should outline the design, viz. “chart reviews”, retrospective patient record study, descriptive analysis, logistic regression analysis, etc.

Response 2: We have accepted this comment and modified the paragraph.

3. The last line of paragraph 1 in Discussion section – Will it be possible to compare/discuss results obtained from any existing demographic surveillance system (DSS) in Ethiopia? The authors may want to contact with Butajira HDSS to validate the results (reference: Berhane Y, Wall S, Fantahun M, Emmelin A, Mekonnen W, Hogberg U, Worku A, Tesfaye F, Molla M, Deyessa N, Kumie A, Hailemariam D, Enqueselassie F & Byass P. A rural Ethiopian population

Response3: We have accepted this comment. We are working together under the same university with the Butajira DSS site and we have also national network with other 5 DSS sites in the country. But still there is no strong evidence showing the epidemiological transition in terms of communicable and non communicable diseases.

4. Under Discussion section (on limitation of the study), the authors need to include the quality of hospital records with citations.

Response4: We have accepted this comment and modified the first two paragraphs with references.