Reviewer's report

Title: The burden of disease profile of residents of Nairobi's slums: Results from a Demographic Surveillance System

Version: 1 Date: 14 June 2007
Reviewer: Rosana Norman

Reviewer's report:

General

This is an interesting manuscript documenting the burden of disease profile of residents of Nairobi’s slums. The authors use a verbal autopsy approach to fill the gap left by lack of vital registration data in their local setting. Although global burden of disease estimates are available for the African region, the authors focus on quantifying the disease burden facing the urban poor providing important and relevant information for local health planning showing that most of their health needs could be addressed by proven cost-effective interventions. This study makes a useful contribution towards filling the enormous gap in cause of death data in the sub-Saharan African region.

The methods are appropriate and well described and the authors made reasonable assumptions to overcome the shortcomings of available cause of death data to arrive at estimates of the burden of disease using data from the Nairobi Urban Health and Demographic Surveillance System (NUHDSS). It is difficult to assess fully whether the data are sound and well controlled from the manuscript. More detail is required on quality control regarding the DSS rounds and it would be helpful to present more information about the death rates observed. Please see Minor Essential Revisions. The manuscript adheres to the relevant standards for reporting and data deposition and the material is presented clearly and appropriately although some suggested improvements can be made. The discussion and conclusions are well balanced and adequately supported by the data.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1) In both the Abstract (pg 2 last sentence of the Background section) and in the last paragraph of the Background section on pg 5 the authors refer to the Burden of Disease approach. The authors should consider changing this to a Burden of Disease approach as there is a growing range of approaches that may be considered Burden of Disease. This is relevant since the authors are not carrying out a substantive data validation/checking consistency against other data sources etc that would form part of a national burden of disease study.

2) In this manuscript, mortality burden and YLLs per 1000 person years are referred to as mortality risks and the terms are used interchangeably. The authors should use only the term mortality burden instead of mortality risk. In order to compare mortality risks it is necessary to calculate the probability of dying between two specific ages.

3) Abstract
The first sentence of the Results section does not read well, please modify.
In the second sentence, this should be overall burden instead of mortality burden in DALYs per 1000 person years.
In the third sentence of the Results section the authors should replace ‘four times the risk of dying’ with ‘four times the mortality burden (or premature mortality rate if YLLs/1000 person years)’.
There are 2 conclusions in the Abstract

4) Methods
Pg 6 The NUHDSS
More detail on quality control regarding the DSS rounds should be provided. It is difficult to obtain a sense
of the extent of missing verbal autopsy data, and how many visits were made to each home when residents were not present.

5) Pg 7 Cause of death ascertainment
Causes of death were classified according to ICD-10 but it is not clearly mentioned if these ICD 10 cause of death classifications were mapped to the global burden of disease cause list. Following the GBD hierarchy, a disease category should not appear in a ranking list of diseases for eg central nervous system disorders (which is not a GBD disease category), injuries (unintentional and intentional categories combined so a broad group) are ranked with conditions or single causes of death such as malaria in Table 3. The same applies to other perinatal causes in Table 2.

6) The process of coding underlying causes of death involves some extent of misattribution or miscoding even by medically qualified staff. The authors should please discuss possible incorrect or systematic bias in diagnosis if, for example, the cause of certain deaths in children could not commonly be ascertained and were therefore coded as undetermined. The problem of misclassification could be addressed by redistribution of general ill defined categories, and a correction algorithm for re-classifying undetermined causes of death could be developed. Although a small proportion of mortality burden (7%) was undetermined, the authors could consider some redistribution of the undetermined causes of death (by age and sex). This would impact on the rates calculated.

7) Pg 7 Burden of disease estimation:
The authors should also indicate how stillbirths were incorporated into estimates of burden of disease and which modifications of the DALY formulation were used so that YLLs could be assigned to stillbirths.

8) More detailed information is required on the YLD calculation. YLLs were multiplied by the corresponding YLD factors for the Afro E region. It is not clear what is meant by YLD factor. It is also not clear whether the authors used Afro E age-specific ratios of YLD/YLL to estimate YLDs by multiplying the YLLs for each cause by the corresponding ratio. Please briefly explain why YLD “factors” for persons rather than sex-specific ones were used. Even if estimates for males and females are later combined, it would be useful to estimate YLDs using age-sex specific ratios of YLD/YLL. A reference is necessary for the Afro E 2002 estimates.

9) Pg 7 All deaths were distributed to sex, age groups- should this be All deaths were classified to sex, age groups?

10) Pg 8 The intervention addressable burden
Please consider presenting more detailed information on the HIV/AIDS and Sexually Transmitted Infection (STI) care intervention.

11) Results
A basic description of the mortality would be useful as it would add value to the paper for comparison with other settings and it would also enable better assessment of the robustness of the data and results. The mortality rates should be presented in age groups to assess the validity of the data and whether, for example, child deaths are missing.

12) Pg 9 Burden of disease estimates
It is not clear how the largest contribution from the 15-64 years age group reflects the wide range of ages in this group.

13) On pg 11 Estimates of the intervention addressable burden
Disease conditions that account for 81% of DALYs in the study population can be addressed by available proven cost-effective interventions. Then on pg 12, first paragraph of the discussion, this is reported as 80%. On pg 12 the authors acknowledge that there is overlap and that the percentages addressed by different interventions do not add up to 100%. However, it is not clear how the authors calculated this 81% from the proportion of disease burden among the under 5 and 5+ populations presented in Figures 4 and 5, respectively. It would be useful if the authors could briefly describe steps that were taken to avoid double counting when calculating the joint disease burden addressed by the combined interventions?

14) Pg 11 AIDS and Tuberculosis were combined in the analysis because about 35% of deaths were due to a probable combination of HIV/AIDS and TB and hence were coded as unspecified TB. In Table 3 “AIDS and TB” combined accounted for 35% of burden 5+. In Figure 5, how was the AIDS and TB burden then teased out into the proportion addressable by HIV/STI Control (17%) and TB DOTs (9%) interventions. What was done to avoid double counting?
In adults, verbal autopsy can be used to make reasonable estimates of the number of AIDS deaths, but the authors could discuss whether this technique has been well validated in children. Pneumonia and diarrhoeal diseases are listed as the leading causes of disease burden in under-fives. Given the high prevalence of HIV in the area, the authors should discuss possible misclassification of AIDS deaths in children to indicator conditions such as pneumonia and diarrhoeal diseases. In South Africa, Groenewald et al. looked at the age distribution of the increase in cause specific death rates, and there was extensive misclassification of AIDS deaths into 9 indicator conditions (TB, diarrhoeal diseases, lower respiratory infections etc). [Groenewald P, Nannan N, Bourne D, Laubscher R, Bradshaw D. Identifying deaths from AIDS in South Africa. AIDS 2005; 19:193-201].

On page 13, it would be easier to compare mortality rates in Tanzania with those in the study population rather than premature mortality (YLL rates). Again on page 16, age-standardized road traffic and interpersonal violence injury mortality rates would be useful to enable comparisons with other African countries using the GBD categories of external type of injury rather than mode/mechanism of injury such as blunt trauma or gunshot wounds.

Mortality contributes about 71% of total disease burden which is lower than the contribution of mortality in sub-Saharan Africa. Were comparisons made with the GBD 2002 estimates (reference 23) or 1990 estimates? From Table 1 it seems that authors should replace reference 14 with 23.

Inability to assign causes of death in 7% of deaths would not only lead to an underestimation of YLD and DALYs, but YLLs and rates for specified causes would also be underestimated.

Figures and Tables
Figures are not correctly numbered (all Figures are labelled Figure 1).

Table 1 Title is too long and confusing - can be improved
In all Tables, authors should consider removing the decimal place from YLL, YLD and DALY estimates (but leave 1 decimal place for %).

Figure 1: The population pyramid for the study population needs a scale for the percentage of person time.

Figure 3 legend should be: Age-specific premature mortality rate expressed as YLL per 1000 person years, Nairobi DSS, 2003-2004.

The authors could consider hyphenating sub Saharan (sub-Saharan) Africa throughout the manuscript.

In Kenya this proportion (rather than percentage) is about 71%.

In Methods the authors refer to WHO-AFRO E estimates which are thereafter referred to as sub-Saharan African estimates. When comparing to sub-Saharan African estimates on pg 10 of the Results the authors could perhaps mention that these refer to WHO AFRO-E 2002 estimates to avoid confusion with sub-Saharan African estimates from GBD 1990 in subsequent discussion.

In addition, the authors could consider adding a recommendation in the discussion section to improve availability of reliable data and improve vital registration systems.

What next?: Accept after minor essential revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.