Reviewer's report

Title: A study of trends in access to water supply and sanitation in 31 major sub-Saharan African cities

Version: 1 Date: 31 October 2013

Reviewer: Guy Norman

Reviewer's report:

GENERAL REMARKS

This is an interesting study of a deeply interesting topic: as noted by the authors, improving urban water and sanitation has profound implications for public health in Africa. I believe this work should be published. However, I would prefer a more up-front recognition of the probably severe limitations of DHS data for comparative tracking of progress in urban water and sanitation. The limitations are diverse, but include likely variation among cities in the ways in which "improved" service levels are defined, and in the extent to which DHS surveys include settlements lying within the agglomeration but outside the formal city. A possibility would be to re-frame the report with a title along the lines of "Trends in access to water supply and sanitation in 31 major sub-Saharan African cities: what can official survey data tell us?" And in line with this, I would like to see clearer and more explicit recommendations about ways in which these trends might be more accurately explored in future. Once we have plausible data on trends, we can start to think about factors that might be affecting those trends! So to conclude: this is very interesting research and I would like to see it published, but I think there needs to be much clearer recognition of the likely limitations of DHS data.

STANDARD REVIEW QUESTIONS

Is the question posed by the authors well defined? Please see my introductory comment.

Are the methods appropriate and well described? Yes, but see my introductory comment. Given the data limitations, some sort of triangulation process (e.g. cross-check against other types of survey data, or structured "sense-check" of findings by a panel of experts) would help; though I don’t suggest this as a requirement for the present report. I would however prefer to see in the present report, if possible, some assessment of the extent to which each survey covers the whole agglomeration.

Are the data sound? Yes, but see my introductory comment.

Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes.

Are the discussion and conclusions well balanced and adequately supported by the data? Yes, but see my introductory comment.
Are limitations of the work clearly stated? Yes, but see my introductory comment. [The limitations of DHS data are rather extensively discussed in the Discussion, but these limitations are in my view not adequately recognised in the framing of the report.]

Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes.

Do the title and abstract accurately convey what has been found? Yes, but see my introductory comment; also Comment 5 below.

Is the writing acceptable? Yes, though see some minor editing suggestions below.

SPECIFIC COMMENTS (ALL RELATING TO DISCRETIONARY REVISIONS)

1) I would tentatively suggest that the article should be edited to re-frame it as an exploration of the usefulness of DHS data for tracking trends; this does not preclude trends being reported where the authors find that the DHS data give them confidence; but it highlights that the DHS data likely have severe limitations. This re-framing could be through i) a title change; ii) changes in wording of the last paragraph of Introduction defining study aims; iii) sentence-level wording changes throughout, along the lines of [old] “sanitation access in City X is increasing” to [new] “the DHS data suggest that sanitation access in City X is increasing”; iv) a new subsection in the Discussion section explicitly querying the plausibility of DHS data, perhaps including some or all of the text currently under “Strengths and limitations”; and/or v) a new subsection within the Discussion section explicitly providing recommendations for improved comparative tracking in future (including both recommendations on how to use existing published data, and recommendations on how more plausible data might be collected, whether in DHS surveys or perhaps more feasibly in other more specific research/evaluation initiatives). I treat all these suggested changes as “discretionary revisions” because I’m suggesting a rather significant re-framing, which the authors might not agree with.

2) Although extensive triangulatory sense-checking of the plausibility of DHS data is beyond the reasonable scope of this work, the authors might perhaps have time to select a few example cities and have a think about whether the DHS data are plausible. Just one example: the DHS data indicate that 99.9% of the population of Addis Ababa has improved water supply, but a 2008 survey of three poor areas of Addis suggests that this is unlikely to be the case (Sharma & Bereket 2008, “Water supply systems in selected urban poor areas of Addis Ababa, Ethiopia”; see http://wedc.lboro.ac.uk/resources/conference/33/Sharma_SK.pdf). Such a quick analysis would provide a basis for initial assessment of the utility of DHS data, and for recommendations as to how to track trends in future.

3) I would like to see, if possible, inclusion of some sort of assessment of the extent to which each DHS is covering the whole agglomeration (i.e. the agglomeration whose population is given in UN-DESA, i.e. Reference 1) or only a
part of that agglomeration (e.g. the central municipality). We might suspect that a 
DHS survey considering only the central municipality will tend to suggest better 
coverage than a survey covering the whole agglomeration. I do however 
recognise that it might not be straightforward to characterise the DHS surveys in 
this way.

4) The focus of the text of the Results section is very much on progress, as opposed to current situation. But I think current situation is also of interest: apart from anything else, any assessment of trend is arguably only as good as the two assessments of current situation at each end!

5) The Results section on independent variable effects is wordy and hard to follow: could we get some sort of quick graphical representation of which effects appear significant? It would be good to include some sort of summary of effects in the Abstract.

6) Figures 1-4 are excellent, very clear. Can I just suggest that the captions refer readers to Table 2 for the actual numbers?

7) Various minor points. Are all your countries “low-income” on the WB classification, or are some “lower middle-income”? The paragraph starting “In measuring access to WS&S” is a bit hard to understand, it’s not immediately clear that you’re talking about the four measures already listed. The sentences “Some of these factors are included...” and “The presence of water supply and sanitation policies was considered...” are initially confusing, need to make clear the distinction between considered and selected for inclusion in the quantitative analysis. You mention small sample size as a study limitation: I’m not sure this is a limitation, you’ve sampled 100% of your universe. Text needs checking to eliminate typos (e.g. dependant, helminthes), and References list needs attention to formatting; please note that I haven’t checked correspondence between references in text and in the References list.

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.

Declaration of competing interests:

It is my understanding that one of the authors of this paper has been involved in the award of a research grant, to a consortium within which my current organisation (WSUP) is a sub-grantee. It is my understanding that the award decision had already been reached before I reviewed this article.