Reviewer's report

Title: Changes in Healthy Life Expectancy and the Correlates of Self-rated Health in Bangladesh between 1996 and 2002

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Reviewer: Emmanuelle Cambois

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Essential Revisions needed

General comment

I appreciated the care taken by the authors to address questions and suggestions. Most changes or clarifications are fine but I am not so convinced by few points and remain sceptical about the accurateness of comparing results in 1996 and 2002: there were changes in questions which might really have an impact (see below 1.) and, in parallel, the results are surprising, while massive differences (improvement?) in health is found is contrasting with the increase in mortality, with no justification for this (see below 2.). In the meantime, the sample composition differs in the two surveys (see below 3.). So, in a published version of this paper, while the analyses bring important information showing part of life with health problems and variations according to various socio-demographic factors, I would recommend that the authors explain or discuss more (be more critical with?) these differences and nuance their conclusions.

=> In this line, I would also recommend not to mention "changes in HLE" in the title and presentation of the results, but rather to include a discussion to consider whether they could be interpreted as changes or not.

Detailed comments

1. Changing in response categories for the self-perceived health question remains for me an issue. I don't understand why the fact that the response are not scale but are categories should make this point less problematic. I still think that an option is that everyone recalibrate according to the new categories. See paper Jürges et al, Are different measures of self-rated health comparable? An assessment in five European countries. European J Epidemiology, 2008) 23:773–781. The author cannot really neglect the possibility of an effect; at least, the arguments provided (both the fact that it is not a scale and the specificity of the translation) are not sufficient in my opinion.

=> Should the authors consider that the larger prevalence of good SRH obtained with the 2002 survey might be partly due to this change in reporting health level and therefore nuance their conclusion?
2. Indeed, the explanation of an improvement in health provision to interpreter
the massive improvement within 6 years appears somehow problematic while in
contradiction with the increase in mortality over age 25 for men and stagnation
for women. It appears also problematic when observing a change in the sample
structure which includes a larger proportion of illiterate, poor, low educated in
2002. The health improvement should have been so large, considering it should
also have offset the "unfavourable' change in the sample structure (see next
point).

=> Do the authors have strong argument to explain opposite direction of health
and mortality changes?

3. Changing sample’s characteristics is also an issue: are the differences in the
illiterate, poor, low educated in the two samples representative of the population
dynamics within this period? It is also odd that the "non religious" go from 15% to
3% of the sample (increasing "religious" category, but massive increase in
"missing" category).

=> Do the authors have an explanation for these sample composition
differences? If it does not represent actual population dynamics, I would be
reluctant to comment too far this variable with such a change in its share, when
taking "non religious" category as reference in the model. The multivariate
models shows that changing sample composition might have influenced the
associations: at this point one can wonder whether changing findings also results
from a changing sample effect.

Moreover, in the background section I would be also more nuanced on the
explanation for the association of health and religiosity: in addition to a possible
protective effect of religiosity, it might also be a structural/social inclusion effect:
1) it might depends on the average level of religiosity in the population (or
sample) and whether people are part of the majority or of the minority when they
are religious or not; 2) and the social composition of these 2 groups. To go in this
line, the changing distribution from 1996 sample to 2002 sample makes this
variable not anymore significantly linked to SRH in the second sample.

4. Regarding the response-rate and coverage, 95% is a very high response rate.
Although I would also consider that when refusals are due to health problems,
the fact that interviewees go to the next household to administrate the survey
questionnaire would not solve the under-representation of the ill people:
interviewees might replace sick people who cannot or refuse to participate by
healthy people who can and accept to participate. I am not saying that this
should be largely discussed in the paper but this should be kept in mind when
thinking at the quality of the collected data in any country, and be aware of the
impact of possible changes in this respect from one survey round to the next one.

5. Despite the authors comment the fact that the relationship between life
satisfaction and health can be in both causal directions, they keep mentioning
that their study show a positive impact of life satisfaction on health status. I
recommend rather mentioning a positive association rather than an impact, as
the association can be also largely due to a higher propensity for those in poor health to be less satisfied with life. I don't see why the tests proposed should confirm a causal impact of one towards the other.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

'I declare that I have no competing interests'