Author's response to reviews

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

Authors:

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Version: 2
Date: 3 August 2014

Author's response to reviews: see over
Additional editorial request:

'Please include the contextual information of your study in the Background section of the Abstract.'

We have revised the abstract (p. 2).

Reviewer: Domantas Jasilionis
Reviewer's report:
The authors provide new and interesting insights on healthy life expectancy and its determinants in Bangladesh. The paper is well written.

Thank you very much for your valuable and positive comments and suggestions.

However, there are few shortcomings.

1) It would be much more interesting if the authors also consider disability- or chronic illness adjusted life expectancies. However, this is not possible due to the data restrictions. In this case, the authors should provide at least some references to other studies examining determinants of the objective and self-reported health status in Bangladesh (if any).

We have reviewed recent literature on mortality, self-rated health, and health expectancy in Bangladesh, and we have cited these works in our revision (pp. 5-9).

2) The estimation of healthy life expectancies are based on the Matlab mortality data and prevalence data from the World Value Survey (WVS) data. Besides acknowledging shortcomings of the WVS data, the authors should provide more information about the Matlab data (coverage and other characteristics).

As vital registration data was inadequate in Bangladesh, the data collected under the Demographic Surveillance System maintained by the ICDDR,B in Matlab thana (thana, along with upazila, is a sub-district in the administrative divisions of Bangladesh) have been used to prepare nationally representative regular life tables. The life tables (from 1974 to 2007) can be obtained from the Human Life-Table Database. We used the same source for the 1996 and 2002 life tables. On page 10, we revised the text as follows:

Age- and sex-specific standard period life tables for the years 1996 and 2002 were obtained from the Human Life-Table Database [1]. Using the vital statistics collected under the Demographic Surveillance System in Matlab thana, nationally representative life tables were produced by the International Centre for Diarrhoeal Disease Research, Bangladesh. Bangladesh is divided into 64 districts. The districts are further divided into subdistricts called upazila or thana.

We discuss the shortcomings of the WVS, e.g. limited sample size without data on the institutionalized population, self-rated data, etc. in the limitations section of the study.
Issues regarding participation rates and coverage of WVS were raised by one of the reviewers - Dr. Emmanuelle Cambois. We discuss this issue at some length in our response to Dr. Cambois’ comments. Please see our response.

3) The description of independent variables (4 pages) is obviously too long and should be shortened.

The reviewer is right in that the description of the independent variables was long. As both the rationale for and measurement of the variables are important, we have reorganized the description of the independent variables. The rationale for the independent variables (i.e. the literature review) is now in the ‘Background section,’ and discussion of their measurement is in the ‘Independent variables’ section.

Dr. Emmanuelle Cambois suggested reviewing more literature as the rationales for including some of the independent variables are too brief. We have therefore reviewed recent literature and cited these works in the Background section (pp. 5-9).

**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:** I declare that I have no competing interests.

Thank you very much.

**Reviewer: Emmanuelle Cambois**  
**Reviewer’s report:**  
This paper provides first estimates of HLE for Bangladesh for two dates 1996 and 2002. The analysis uses an internationally designed survey comprising information on health, values and social situations run in 1996 and 2002. Data on self-rated health are combined with regular life table of the corresponding years for men and women separately. Additional analyses are run to highlight correlations between SRH and various socioeconomic and values variables. Observed changes in the health determinants are discussed.

The study indicates that healthy life expectancy has increased. The analysis indicates that some advantaged groups (education, ...) happened to have gained less than more disadvantaged group between 1996 and 2002; moreover the gain difference was such regarding literacy that the illiterate group reports better health than the literate group in 2002. These results are unusual and unexpected and should be discussed or documented. Indeed, in the meantime, life expectancy did not increase so much for women and even decreased for men; this could be part of the explanation. And in parallel, there are some survey issues that could interfere. (see below).

This is a valuable study because little information is available on health in this country and health expectancies are useful indicators combining mortality and health patterns. Meanwhile,
further information, in particular looking at survey issues, could help comforting the outcome and improving substantially the relevance of the paper. If survey issues happen to be source of bias regarding trends analysis, this should be known and discussed. Other explanations of the observed trends need to be addressed.

Thank you very much for your valuable comments and suggestions.

Major revisions

First, I would argue that points of major interest concerning Bangladesh is the massive increase in life expectancy over past decades, as mentioned in the introduction, but further is the recent stagnation in LE or even decrease for men. This latter pattern is mentioned but very late in the paper; while it makes the motivation of the paper even more relevant in my point of view: How this trend articulates with health outcomes? How far increasing mortality impacts the whole population or a selection of it? Health expectancy is a useful tool to explore this. Furthermore, it could be part of the explanation of the results obtained.

We have revised the ‘Background’ section as suggested by the reviewer (pp. 4-6).

Indeed mortality seems to have declined rather at older ages inducing, at least for men, that the gains in LE were made at younger ages at which ‘good SRH’ is the highest. I would suggest developing more this aspect: it is mentioned in the text that mortality should have increased at older ages, but more importantly it needs to be mentioned by which mechanism: were older people (whole or part of) more exposed to adverse conditions over the recent period? Is this been more pronounced for the most deprived (did the SES distribution of the population at older ages has changed over this period)? If such a process explains the mortality increase, then it could also explain a stronger selection effect and an increase in the healthy years for the survivors who might be more robust. This could be a way of interpreting the unexpected outcome of increasing HLE and increasing good SRH in all groups especially among the disadvantaged. Therefore, I would suggest putting forward this information and further document it, at least in terms of age impacted as mentioned in the discussion.

We have highlighted and further documented male mortality in the ‘Background’ section as part of the context (pp. 4-6).

Second, there are survey issues:

> I think the authors needs to provide more information on the 1996 and 2002 samples (at least participation rates) to ensure the validity of the results or to highlight possible artefact in the observed trends. While the sample size is limited, its representativeness regarding health and social factors is an important issue. This information should be checked and discussed as this might compromise the trends analysis. Determinants of non-participation to surveys are known to be associated with social status and poor health; a reduction in the participation rate might go with a stronger selection on both health and social factors (and higher probability of good health). Changes in the sample coverage might participate to the outcomes and it is good to know if the coverage is high and stable from 1996 to 2002.
Although the technical report for Bangladesh of the 1996 WVS explained the sampling procedure (a multistage random sampling technique) and pointed out that care was taken to ensure appropriate representativeness of the sample, it did not mention the non-response rate. There are two possible reasons for not specifying the non-response rate. First, the 1996 WVS reached the full sample size targeted. The 1996 WVS followed the same procedure as the 2002 WVS. In cases of respondents refusing to participate, interviewers moved on to the next household picked according to the sampling procedure and collected data. And second, the non-response rate was negligible and therefore not worth mentioning.

The non-response rate mentioned in the sampling and methodology of the 2002 WVS for Bangladesh is 5%. But the 2002 WVS reached the same sample size targeted; in cases of respondents refusing to participate, interviewers moved on to the next household picked according to the sampling procedure – i.e. interviewers went to the next 5th household and collected data.

The sample coverage was 100% in 2002. We do not know about the 1996 WVS sample coverage. It seems that the sample coverage was 100% in 1996 as well, because the fieldwork for both the 1996 and 2002 surveys was undertaken by the same institution- Bangladesh Unnayan Parished, Dhaka.

As we do not have data on participation rates for the 1996 WVS, we are unable to mention it. We have mentioned the response rate as 95% for the 2002 WVS (p. 10).

> The data does not cover institutions: is this a major issue in Bangladesh? Did the % of institution residents’ changed from 1996 to 2002? This could be documented

This is not a major issue in Bangladesh. Unfortunately, there is no data and/or literature on institutionalized residents; we are unable to document the issue raised by the reviewer.

We have included the following statement to the ‘Limitations’ section of the revised manuscript.

If individuals living in institutions have poorer health than individuals residing in the community, not taking into account the institutionalized population might overestimate HLE, especially at older ages [3]. Here, we assume that people living in institutions exhibit the same distribution of health conditions as people in the community at large.

> The change in the response categories for self-rated health might have had an impact: in 2002 the category "very poor health" has been suppressed. The authors implicitly assumed that the changing category only impact those with 'very poor health' who would rate themselves in "poor health" in 2002. Meanwhile whether this missing category induced all people to over-rate their health in 2002 is an option; the whole distribution between good-average-poor health might have moved towards better health as interviewees calibrate their answer relative to the response categories scale; changing the scale might change the individual calibration. Here again, this has to be discussed as it might compromise the trends analysis.
Thank you very much for pointing this out and for your clear explanation. However, because the respondents calibrated their answer relative to the response categories (very good, good, fair, poor, or very poor) and not the scale (from 1 to 4/5), this is not the case with our study. We discuss the issue in the study’s ‘Limitations’ section as follows (p. 22).

…the response category ‘very poor’ was absent for the question of SRH in 2002. This missing category may have induced study respondents to over-rate their health in 2002. But it is unlikely, because respondents calibrated their answer relative to the response categories (very good, good, fair, poor, or very poor) and not the scale (from 1 to 4/5)…. So, it is reasonable to assume that the changing category only impacted those with ‘very poor health’ who would rate themselves in ‘poor health’ in 2002.

Question/suggestions:
Page 4: as mentioned above, recent trend in mortality could be presented and documented in introduction, in particular, exposing that LE loss concern older ages, at which poor health is more prevalent.

We have revised it. Please refer to the response above.

Page 4 (end of line 5): I think it should be written "Health expectancy can be measured by a variety of different health dimensions" (and not LE can be measured...). I suggest also that this sentence comes at the end of the paragraph. Indeed, the following sentences discuss the health expectancy indicators in general and not the HLE in particular.

We have revised the points as suggested by the reviewer (p. 4).

Page 5: In Mathers et al., I wonder whether the indicator used is a HLE (the health expectancy based on SRH) or another indicators (health or disability adjusted indicator as the paper refers to the GBD project). This should be clarified.

As Mathers et al. reported a disability-adjusted indicator, we have dropped the statement from our revision.

Page 5: Figure 1 is not so useful.

We have dropped Figure 1.

Page 6: Participation rates to the Bangladesh survey is needed to discuss possible bias as mentioned above.

Please refer to our response above.

Page 7: Discussion about change in response category for self-rated health is needed as mentioned above.

Please refer to our response above.
Page 7 (3 lines before last): the age groups and independent variables rather refer to "SHR" than "health expectancy calculation"?

We have revised it (p. 11).

Page 8: Rationales for analysing the various explicative variables are too brief, especially regarding the link made between religious faith and diseases of the central nervous system. All the variables used are indeed interrelated and associated with health altogether. This should be better exposed. Is religious faith in Bangladesh linked to education, locus of control, income and life satisfaction? Did this changed over time (or did the sample distribution changed in this respect)? This needs to be discussed especially because the results are counterintuitive regarding what is explained in this paragraph: if religious faith tends to increase comfort and to reduce stress, religious should be associated with relatively better health, which is not what is found if I understood well.

We have reviewed the recent literature and cited these works in our revision (pp. 5-9).

Dr. Domantas Jasilionis, the other reviewer, suggested shortening the description of the independent variables. As both the rationale for and measurement of the variables are important, we have reorganized the description of the independent variables. The rationale for the independent variables (i.e. the literature review) is now in the ‘Background’ section, and discussion of their measurement is in the ‘Independent variables’ section.

We have checked religiosity in terms of age, sex, education, control over life, income, and life satisfaction, and in our discussion we have documented why we have a negative relationship between religiosity and SRH (p. 21).

Quality of the reported information can be discussed: is income known to be accurately documented? Etc...

We discuss this in the study’s ‘Limitations’ section as follows: Due to the unavailability in the data sets of the exact income of respondents, we had to rely on the income scale.

Page 14 (first line): here the reader finds out that LE actually decreased for men, in spite of what is said in introduction about the massive increase in LE. Therefore, this recent trends should be mentioned in introduction as discussed above.

We have revised it as suggested by the reviewer.

Page 15-16: here unexpected results are presented showing a greater increase in good health for the most disadvantaged and for the religious, ending up with some "inverted" association with health. And Page 17 (first paragraph), the authors highlight the fact that life satisfaction is the only variable remaining associated with health in both surveys. I wonder whether the authors could have rather proceeded in two steps with univariate and then multivariate analysis, rather
than correlations, to compare the association with health: the variables are all interrelated and the SRH prevalence might be strongly related to age structure and social structure of these groups: Are the religious with poorer health because they are mainly older people? The authors suggest this, but then it could be more informative to see whether the associations are changing from univariate to multivariate models.

We perform univariate analysis this time and present results in Table 5 (p. 37). We have revised the text accordingly (pp. 17-18).

As suggested by the reviewer, we have dropped the results and explanation of chi-square and correlation tests (previously presented in Tables 4 and 5). We renumbered all tables and revised the text accordingly.

We have not dropped the statement of chi-square and correlation tests from the ‘Statistical analyses’ section, because the variables significant in chi-square and correlation tests at level p<0.20 were included in the multivariate analysis.

We ran some cross-tabulation analyses to check issues associated with religiosity. We discuss them in our revision (p. 21).

The association with life satisfaction is interpreted as a major driver in the association with health. But here again I would say that changing population composition (and/or sample coverage?) might lead to a changing association in the model rather than a changing influence of life satisfaction on health.

Actually, we wanted to say that life satisfaction is the significant correlate of SRH in both 1996 and 2002. And improving life satisfaction could play a vital role in improving SRH directly and HLE indirectly.

Page 18: as suggested above the increase in male mortality can be highlighted in introduction as part of the context. This might be an explanation of the growing HLE if the increase in mortality has impacted the most deprived and less robust groups.

We have highlighted male mortality in the ‘Background’ section as suggested by the reviewer.

Page 19: Life satisfaction and the other variables are associated with health. The authors suggest that these variables impact health, but health impacts all these variables. This is mentioned in the conclusion but should be discussed here. Furthermore, this section explains possible interaction between variables and this could be document by univariate+multivariate analysis as suggested above.

As suggested, we have revised the issue (p. 21). We have taken the statements of life satisfaction from the conclusion and placed them here. The possible interactions between variables were tested and found not to be significant.
Page 20: the limitations should be further discussed to see how much data quality/reliability issues could impact the results. Not only regarding the sample and the health measure, but also regarding the other variables such as income. Are these surveys brings coherent outcome with respect to what is known on the population structure and from other data sources?

In the Limitations section, we address other shortcomings, such as the institutionalized population, categories of SRH, and income.

The current study brings coherent outcomes in terms of HLE. But in terms of correlates of SRH, this study used a different set of variables (except socio-demographics) than existing research. And as a result, we have a new finding that life satisfaction is a significant correlate of SRH.

**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.

Thank you very much.

**Additionally**

We have added a small table- Table 1 for life expectancy by age and sex in 1996 and 2002 (p. 33).

We felt that previous Endnote 1 was redundant and so have removed it.

We have added the statement ‘For more details on computation of HLE and confidence intervals using the Sullivan method, see Jagger et al. [55]’ on page 15.

We have revised the list of references (pp. 25-32).