Author's response to reviews

Title: Gender and socioeconomic disparities in BMI trajectories in the Seychelles using birth cohorts generated from serial population-based surveys

Authors:

Isabelle A Rossi (isabelle.rossi@chuv.ch)
Valentin Rousson (valentin.rousan@chuv.ch)
Bharathi Viswanathan (bviswanathan@gov.sc)
Pascal Bovet (pascal.bovet@chuv.ch)

Version: 2 Date: 15 July 2011

Author's response to reviews: see over
Dear Editor,
We submit a revised version of our manuscript. Following a recommendation by a reviewer, we have performed additional statistical analysis of our data. This has provided new and interesting insight on the data and we have subsequently markedly modified our paper. We believe that we could address all the issues raised by the reviewers and the revised paper is much improved.

Answers to the Reviewers' comments:

We thank the Reviewers for their helpful comments and suggestions. We have addressed the issues raised as follows.

Reviewer 1 (Jessica Jones-Smith):
Thank you for the opportunity to review the manuscript “Gender and socioeconomic disparities in BMI trajectories in a middle-income country in the African region: a cohort analysis”. This work has the potential to be an interesting contribution to the field, but its current form, it lacks sufficient detail to be able to evaluate the methods, results and conclusions.

Specific comments:

Abstract:

1. Abstract should be revised to give sufficient detail on the study design, N, and statistical methods.

We have clarified the methods section and added further details on the surveys from which we derived our data, including the numbers of participants and the different statistical methods used. Furthermore, following recommendation by the Reviewer to do statistical tests, the text has been adjusted accordingly.

2. In the methods section of the abstract, the term population-based survey is vague. It is unclear whether these 3 surveys are repeated cross-sectional surveys or whether this might be a longitudinal cohort or panel study. Are some participants surveyed in all three years? What is the N for your study?

We have added the total numbers of participants in the 3 surveys and that each of these surveys included random samples of the population (hence each survey was independent and included different participants).

3. Additionally in the methods section, please include a brief description of the statistical methods used to generate the results.

We have further clarified the methods used for the descriptive analysis. Furthermore, we have added a new paragraph on statistical models that we have now used to test cohort and other effects.

Background

4. The Background section lacks sufficient detail to clearly establish what has been done in the field, what the current gaps in knowledge are and how the current study might address some of these gaps.
An updated literature search should produce several more of these gaps. An updated literature search should produce several more recent studies that examine this topic in low- and middle-income countries.

We have substantially reworded the background section and we better describe current knowledge and gaps in knowledge in this area.

5. The cohort analysis should be described in more detail—what exactly is it what type of data are required. Please also provide a reference for this method. A comparison between repeated cross-sectional analyses and cohort analysis and the strengths of the cohort analysis over the repeated cross-sectional analysis should be elaborated on.

Our data come from three repeated cross-sectional design over 15 years, from which we have generated birth cohorts. In presence of a cohort effect, the observed increase of BMI over the participants’ ages would underestimate the true effect of age, since older persons in our samples come from older cohorts who had lower BMI. In this paper, we illustrate how it is possible to disentangle the age effect from the cohort effect. Our method, based on a descriptive analysis and using statistical modeling is now explained in more details in the Method section. We have also elaborate in the discussion section (second paragraph) on how it is possible to disentangle the age effect from the cohort effect using birth cohorts, as compared to results based on isolated cross sectional surveys.

6. The primary research question and potentially some hypothesis should be included at the end of the Background section

We added a more detailed objective of the study in both the Abstract and in the last paragraph of the Background section.

Method

7. The statements in the first paragraph should have references. (page 4)

We added 2 references in this paragraph.

8. Regarding the classification of occupation, what types of occupation are in the intermediate category? Can you provide a reference, for studies that have used a similar classification of occupation?

We have clarified that we used, in each survey, a same question on occupation that provides a six-category classification, consistent with the British occupation-based Registrar’ Social Classes (ref: Methods in social epidemiology. Editors: JM Oakes & JS Kaufman. Jossey Bass Wiley Imprint, San Francisco, 2006). In our paper, we collapsed the 6 categories into 3 categories: the high SES includes “professionals” and “skilled non manuals”, the intermediate category included “semi skilled manual”, “skilled manuals”, “and semi skilled non-manuals” and the low category included “unskilled workers”[4]. We have reworded the relevant section in methods on SES classification accordingly.

9. The methods of the cohort analysis are unclear and need to be described in more detail. The reference to the study design in previously published work is not sufficient to allow the reader to understand how the current study was designed (page 4).
a. The term population-based survey leads me to believe that these are repeated cross-sectional surveys, but it is unclear.

We have clarified that the 3 surveys in 1989, 1994 and 2004 were conducted in independent representative samples of the entire population of the country aged 25-64 years.

b. The description of the cohort analysis leads me to believe that there are some individuals for whom anthropometrics were measured during all three surveys, but this is also unclear.

We have clarified that the three surveys were independent and that there are very few persons who have participated (by chance) in 2 or even 3 surveys based on chance only, as discussed in next point.

c. Please provide the total N for the population-based surveys and the N for the people who were included in all three waves

We cannot know how many participants have participated in more than one survey but we can calculate these numbers. Since the probability for an adult aged 25-64 years to participate in one survey was around 4%, hence the probability to participate in 2 surveys was around 4%*4% ~2 per 1000, we can expect around 30 persons to have participated in 2 surveys and 1-2 persons to have participated in the 3 surveys, based on chance only. We clarified this point in the text.

d. If there are people who were included in all three surveys, how were they identified.

See point above.

10. A more detailed description of the procedures that are required for the cohort analysis is needed. A step-by-step description of the procedure would be useful.

We have described in more details what procedures were used. In addition, as recommended by the Reviewer, we have conducted statistical tests, which are and help to interpret the results and are detailed in the MS.

11. Additionally, more detailed statistical modeling descriptions would be helpful. For instance was linear regression used to model the relationship between BMI and age? Were the predicted BMIs from this linear regression (including second degree polynomials) used in the next step?

We thank the Reviewer for this important remark and suggestion. We have now added statistical modeling in the paper. Hence, the paper now provides two types of analysis/results: 1) descriptive data (using polynomial regression to predict BMI according to age in each gender and each survey, i.e. the association between BMI and age is allowed to be different in each survey- and sex-category) and 2) statistical modeling, for which methods and results are provided in detail in the manuscript. This analytical modeling of generated birth cohorts allows us, now, to test for various interactions, in particular if there is a cohort effect in social patterning of BMI (our main aim in the paper). Results of the relevant statistical tests (which are explained in detail in the text) do not show a statistically significant cohort effect of social patterning of BMI (i.e. there was no difference in social patterning of BMI over time) and we have therefore modified the discussion and our conclusions correspondingly.
12. Is the analysis that generates the SES and sex-specific predicted BMIs based on stratified linear regression models?

Please refer to point 11 above.

Results

13. Since the BMIs are being predicted from the regression models, it seems necessary to assess how well the models fit the data, perhaps by including the proportion of variance explained in each of the models used to predict BMI.

As mentioned in point 11, there are now two types of analyses in the paper: descriptive data and statistical models. The analytical approach that we have added in this revised manuscript allows to specifically testing for different effects. The adjusted R square of our statistical models have been added in the Tables.

14. The results would be more informative if they included numbers, such as the predicted beginning and ending BMIs for each group or for some groups. Also the average rate of change for each of the subgroups would be quite informative.

The new emphasis, in this revised paper, on statistical modeling allows providing objective statements of different effects under consideration in the paper. Also, a lot more emphasis has now been given to methods of analysis (descriptive and analytical) and their interpretation.

Discussion

15. First paragraph: I think it is important to distinguish between mean BMI and the predicted BMIs in the cohort analysis. You indicate that BMIs increased in each successive cohort, this seems clear for the overall mean in Table 1, but not as clear looking at the predicted subgroups levels in the Figures.

All our analyses area based on mean BMI. Our “descriptive data” show mean BMI in relation to age, which is allowed to vary with only few assumptions (i.e. associations between BMI and age are allowed to vary differently in each sex and survey year). On the other hand, the statistical models provide a simplified account of these relationships. Note that the method section and result section provide now a much more detailed account of what is assumed (based on findings on interactions) and how the data should be interpreted in the different analyses.

16. Without numbers presented in the results section (ie beginning and ending BMI for each subgroup and estimate rate of change) it is hard to evaluate statements in the discussion, such as whether BMI increased more rapidly in men and women with low SES across successive cohorts. Also please make the comparison group more clear in each of these statements.

As suggested by the Reviewer, we have now included statistical models which allow testing a series of assumptions and effects and we can now explicitly address our main aim (whether social pattering in
BMI differed across cohorts). Since the statistical model did not support a cohort effect in social patterning of BMI (i.e. social patterning of BMI did not change over time), we have also changed the content of the discussion and our conclusions accordingly.

17. The 4th sentence in the third paragraph of the discussion should have a reference (page 8).

We have added 2 references.

18. The discussion would be improved by specifically addressing the age, period and cohort findings of your study and how these add to current knowledge.

We have re-organized some paragraphs, e.g. the description of the age and cohort effects in the first paragraph of the discussion. Note also that our conclusion has changed after tests performed in this revised MS have shown no difference in social patterning of BMI over time. Hence, the substance of the discussion section has changed correspondingly (towards more conservative and more careful statements).

Discretionary revisions

Abstract

1. The phrase “social gap” is a bit awkward to this reader. I wonder if this could be replaced by social patterning or “differences based on socio-economic status”.

We replaced the words “social gap” with “social patterning” as suggested.

2. The third sentence of the Results section is confusing to this reader due to the phrase “… high than low socio-economic status…” Perhaps using saying “compared to” instead of than or some other wording would improve readability of this sentence.

We have changed the sentence according to the reviewer’s suggestion.

3. The conclusion section of the abstract could be shortened to include only the most relevant conclusions directly stemming from the current study.

We have shortened the paragraph and focused on the most relevant conclusions directly stemming from the current study.

Background

4. Discretionary revisions: there are several more recent studies than those cited that examine the time trends in SES-obesity relationship in lower income countries that the authors have not included in their summary of the literature. In particular Ziraba et al. (2010) specifically investigate this question in African countries.
We thank the Reviewer for this suggestion and we have added the paper of Ziraba et al. (2009) as well as other new references addressing time trends related to SES-obesity.

5. Monteiro et al ask similar question in Brazil. In addition, Jones-Smith et al (2011) examines times trend in SES-obesity relationship in a cross-sectional sample (which includes some African countries).

We have added both references in the Background section.

Discussion

6. The intended meaning of “social gap” must be better defined. It seems that BMI is still socially patterned if high SES men have higher BMIs than low SES men. By social gap, do the authors only mean that the advantaged group (high SES) has better outcomes. Being more specific throughout the second paragraph could help clarify what exactly is meant.

We have clarified the wording of this paragraph. We now clearly account for this social gap in men which can be objectively measured with a statistically significant interaction.

7. Is race/ethnicity correlated with SES in Seychelles and in this sample in particular?

In the Seychelles persons are of predominantly African descent. The small number of participants of non-African descent (Caucasian, Indian or Asian descent) precludes meaningful analysis of trends according to ethnic groups.

Figures

8. Perhaps including the BMI value for each point on the Figures would allow for easier interpretation

Statistical modeling has been now provided, as rightly suggested by the reviewer, and tables provide objective measures for a series of erect. This should help clarify the results-. However, we feel that adding BMI values in the Figures would make the figures too busy and not necessarily help the interpretation.
Reviewer 2 (Samuel Olatunbosun):

1) The paper was well written. This is an important contribution to the epidemiology of Africa, considering the paucity of prospective data in the region. However the socioeconomic structure of Seychelles is atypical for most African populations.

We thank the Reviewer for the nice comment.

Regarding generalizability of the findings in Seychelles, there is vast heterogeneity across populations in Africa. Seychelles is a middle-income country that has experienced rapid socio economic development and has one of the highest GDP per capita in the region. A vast majority of the population is of African descent. The case of Seychelles may anticipate trends in other countries in the region or in other small island states in other regions.

In addition, an important other goal of the paper is to show that data from serial population-based cross sectional surveys can be turned into cohort data. This may be particularly relevant in Africa where very few (if any) population-based cohort data are available for non communicable diseases, but where surveys have been conducted in several countries in the region.

2) A number of the statements in the discussion section, specifically in the second half of the second paragraph, seem speculative in the context of the data provided. Some of the inferences may be valid when previously published data from the three surveys are put into consideration; data to support them from the current study are rather scanty.

Following recommendation of Reviewer 1 to substantiate our findings (and conclusions) with statistical tests, we have now added analytical models of our data, which allows us providing objective statements on the various effects under consideration in this paper. In view of these objective statistical tests, we have now substantially reformulated our discussion of our findings as well as our conclusions. Since statistical modeling actually shows no variation in social pattering of BMI over time, the Reviewer will be glad to note that we have considerably modified the discussion and conclusions toward more conservative statements and we have dropped a number of speculations that we had done in the previous MS.

3) The authors stated (under Methods) that they did not test for differences in the BMI trends among the cohorts, citing the exploratory nature of their analysis. This by itself is a limitation of the study and should be mentioned in the discussion section.

As mentioned above, we have now modeled our data and provided statistical tests for various effects under study. This new approach to our data considerably strengthened our findings.