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

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Author's response to reviews: see over
Answers to the Reviewers’ comments:

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

First, we wish to state that we have dropped our descriptive analysis (which was actually based on a complicated model with an interaction between the period and the age effect, which was ultimately not significant) and we now only present our results from our (simpler) linear regression models. This is based on several reasons: i) there were several issues explicitly or implicitly raised by the Reviewers about this descriptive analysis, ii) the results of our descriptive analysis were essentially the same as the results from our linear regression; iii) the analysis based on linear regression is based on simpler models and is easier to generalize; iv) our linear regression models allowed testing of the effects under study; v) the paper is simplified, shortened and easier to read.

Reviewer’s report

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

Version: 2 Date: 29 July 2011

Reviewer 1 (Jessica Jones-Smith):

Review of revised manuscript: the revised version of this manuscript addresses many of my previous comments; however, there are some important details that require additional clarification.

Compulsory revisions:

Background
1. The flow of background might be improved if the all previous findings specific to Seychelles were moved to the end of the Background, right before the goals of this paper. So that the last paragraph introduces Seychelles as a rapidly developing country with 3 repeated cross-sectional surveys, then lets the reader know what is already known about Seychelles (ie direction of SES-BMI relation for men and women and BMI-age relation) and gives the goals of the current study. We have moved the specific findings on Seychelles to the end of the Background section, before the aims of the current study.

2. Page 3, paragraph 2, first two sentences: It seems necessary to make the distinction here between single cross-sectional studies and repeated cross-sections, which is the category that the present study fits into.
   We have specified when information on the shift of obesity from higher to lower SES groups was made in single or repeated cross-sectional studies.

Method
3. I now understand that these are 3 independent random samples from the Seychelles population.
   This is indeed the case.

4. It seems as though the total number of participants in all 3 surveys is 1585 men and 1818 women. I am still unclear on how many of these participants could be included in the cohort analysis. It seems that a relatively small portion of the total survey in each year would be born in
either 1944, 1954 or 1964, ie in 1989 only people aged 25, 35, or 45; in 1994, only people aged 30, 40, or 50; and in 2004, only people aged 40, 50, and 60.

The N for these cohorts is not provided in the text nor is in the “descriptive” plots in Figures 1 & 2. Is the N for the “descriptive analyses” plotted in Figures 1 and 2 smaller than the full sample? Please clarify.

As stated above, analysis is now based only on regression analysis (i.e. our descriptive analysis has been dropped). Data on all participants were used in all analyses (except for Table 3, where persons of intermediate SES are not included in the analysis for the sake of easier presentation of the SES specific results). Of note, we had referred in the previous revised paper to the word “simple” regression because our linear regression models were “simplified” compared to our descriptive analysis (which was done separately for each survey, thus implying an interaction between the period and the age effects, from which we were estimating the cohort effects). We agree that the presence of 2 types of models in the previous version of the paper was confusing. The current revision is more straightforward to read, while keeping the same findings and conclusions as in the previous revision.

5. Is the entire sample included in the regression analyses reported in Tables 2 & 3, ie including people born not in years 1944, 1954, and 1964? I believe they are, but this should be more clearly stated in the methods. If so, more justification for treating cohort as a linear variable should be provided, ie how was the assumption of a linear relationship between cohort and BMI assessed? This would be different than checking whether cohort/year varied by age (page 5, 3rd para).

All participants are included in Table 2. For Table 3, for the sake of a simpler representation (in particular in view of many more interaction terms when there are 3 instead of 2 categories), we included only participants of high and low SES (in order to have a dichotomized variable for SES and, subsequently for interactions). The “cohort effect” (i.e. the year of birth of a person) is included as a covariate in the model, such that we assume here a linear effect of the cohort (year of birth) on BMI, as rightly noted by the Reviewer. Note that the inclusion of a quadratic term of the cohort would not be significant (p=0.56 for men and p=0.77 for women for the model used in Table 2), as now reported in the paper.

The authors might consider doing a sensitivity analysis in which birth cohorts are formed based on grouping multiple birth years together, such as every 5 or every 10 years. In this way birth cohorts could be modelled as dummy/indicator variables to see if results are similar.

As suggested by the Reviewer, we have also tried to replace the linear effect of cohort by a categorical effect with five possible values (<1935, 1935-1944, 1945-1954, 1955-1964, and >=1965), without obtaining a better fit (for example, the adjusted R2 values were slightly smaller using a categorical cohort effect than using a linear one). Thus, the linear regression model that we use was a parsimonious and convenient approximation of the reality, which was also useful for testing statistically some of our scientific hypotheses.

6. It would be helpful to state at the beginning of the modelling paragraph (p5, para3) then when using this type of data, one can either choose to focus on cohort or period in addition to age due to their collinearity in the data (this is currently stated at the end of this same paragraph). Then, instead of describing the modelling in terms of period and eventually switching to cohort, just alert the reader that you are focusing on cohort instead of period and use the word “cohort” throughout. The introduction of “period” in this modelling paragraph seems to add complication.

We decided to present results in terms of age and cohort effects. We have changed these 2 paragraphs to focus on the cohort and age effects presented in our results and to be more
concise according to Reviewer 2’s comment. We have clarified these issues in several places in the MS.

7. It is not clear to this reader what exactly the goals of the descriptive analysis in Figure 1 & 2 are in the context of the revised manuscript which includes a more elaborate model and also includes more of the sample (I think). Does it provide a check on the more highly modelled results from the adjusted linear regressions? How does this work since different portions of the sample are included in each? Is it meant to be akin to a crude analysis? Please clearly state the goals of the descriptive in relation to the adjusted models. As stated above, our descriptive analysis is dropped in the revised version.

8. The descriptive analysis might be made clearer if it was simply the sex- and cohort-specific mean BMI and the sex-, cohort, and SES-specific mean BMI, rather then using the polynomial regression. These could be reported in a Table instead of Figures. As mentioned above, we chose to drop this part.

9. Page 5, second paragraph: The ages do not match up with the birth year. I believe the 1944 and 1964 ages are transposed. I suspect this is just a typo. The current paragraph states that BMI for those born in 1944, BMI at age 25 was used in year 1989 etc... however people born in 1944 would be 45 in 1989. Thanks you for the remark. The 1944 and 1964 birth years had been actually transposed. However we have cancelled all the paragraphs on this method.

10. In this same paragraph, the word generated seems misleading to this reader. I think it should be made clear that these are “estimated means” based on a model of BMI as a function of a polynomial regression. Unless I am mistaken and the results here are based on a nonparametric estimate of the mean BMI at each age. We agree with the Reviewer that this word can bring confusion. As mentioned before, we have dropped this part of the Methods section, but also from the title of the paper.

11. Page 6, 2nd para, first sentence. This sentence would be improved if the method used to generate Figure 1 was re-stated, ie “based on the sex and cohort specific polynomial regressions”. I would also suggest this for the first sentence of the 3rd paragraph on page 6. Figure 1 has been dropped from the revised version of the paper.

12. Second sentence in this same paragraph:” A cohort effect was evident...”. Please tell the reader how they can tell that the cohort effect was evident, ie because, at a given age, the mean BMI for each successive cohort was higher than the previous cohort. This remark was about our descriptive analysis, which is now dropped from the paper.

13. Figure 1 & 2: If a second degree polynomial regression was used to generate these BMI levels, why draw a straight line through these points? We have now dropped these two figures from the MS.

14. Figures: using different dash lines instead of colour could improve the readability of the figures in print. These graphics are fairly complex indeed. However, we believe that having different dash lines would not necessarily help the reading. What is critically important for the good readability of the figures, though, is the choice of colours and markers. Fortunately, figures can appear in colour in the final printouts in BMC. We would therefore prefer to keep these figures as they are.
15. Tables: Instead of “simple” regression, perhaps “linear” regression.
We have changed the word “simple” in the titles of the Tables with “linear regression”. By “simple”, we had meant simplified models compared to our descriptive analysis, which was actually based on a more complicated model with additional interactions, as mentioned above.

Discussion

16. Page 8, second paragraph: it should be mentioned that the focus on period or cohort is often a necessary a priori choice since these age, period and cohort often cannot all be examined concurrently due to collinearity.
We have made this point explicit in the text, including by adding this sentence in the discussion part. “Ideally, one would like to be able to separate the distinct effects of age, period and cohort, which is however not possible due to their collinearity. With repeated cross-sectional data, one should hence decide a priori whether to model an age effect and a period effect, or to model an age effect and a cohort effect, as done here.”

17. Page 8, fourth paragraph: it might be useful to repeat here that low SES women had higher BMIs as early in 1989 in Seychelles, in addition to the observation that this has not changed over time.
Thank you for the suggestion, which indeed pinpoints to an important observation. We have added the following sentence to this paragraph: “Of note, BMI was higher in women of low than high SES as early in 1989 in the Seychelles.”

Level of interest: An article of importance in its field

Quality of written English: acceptable

Statistical review: Yes, and I have assessed the statistics in my report

Declaration of competing interests: I declare that I have no competing interests.
Reviewer's report

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

Version: 2 Date: 27 July 2011

Reviewer 2 (Samuel Olatunbosun):

The authors have addressed the issues raised during the initial review.

Additional comments:
1. The last two paragraphs of the Methods section are redundant; the points can be summarized into one or two sentences.
   We have now shortened these paragraphs.

2. There are few typographical and syntax errors, examples:
   a. Background, paragraph 2, 2nd sentence - “increasingly” is more appropriate; also 5th sentence - “relationship.”
      Thank you for these suggestions. We have corrected these 2 syntax errors.
   b. Methods, paragraph 2, 10th sentence - check the auxiliary verb and the tenses used.
      We have corrected these errors.
   c. Discussion, paragraph 1 - “Firstly, secondly and thirdly” would be more appropriate.
      We have corrected these 3 words.
   d. Paragraph 5 - Last sentence - “...SES was based on the same questionnaire in all surveys.”
      We have corrected this sentence.
   e. Paragraph 6 - “... when data from the next population-based... “ is more appropriate.
      We have corrected this sentence.

Level of interest: An article of importance in its field

Quality of written English: needs some language corrections before being published.

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.
The manuscript still requires major revision. It was difficult to determine where in the revised manuscript the authors had made the necessary changes (no page numbers or highlighting was provided). This made it difficult to determine whether or not the authors had adequately addressed the reviewer comments. Please highlight the specific changes in the revised manuscript.

The changes made are highlighted in the version of the revised paper, which is provided with “track mode”. We provide both a version in track mode and a version without track mode (as there are many changes in the revised version).

In addition to the major and minor revisions requested by the reviewers I have some concerns regarding the manuscript.

1. The manuscript submitted in my opinion closely resembles a previously published manuscript by the authors using the same data "Prevalence of overweight in the Seychelles: 15 year trends and association with socio-economic status" Obesity Reviews, 2008. For example the objective of the submitted manuscript was to examine the effect of SES (occupation) on BMI according to sex and calendar period while the objective of the published Obesity Review article was to examine the trends in prevalence of overweight and obesity between 1989 and 2004 and their association according to sex, age, and occupation. Can the authors please describe how this manuscript differs from the previously published manuscript?

   The data used in this manuscript come from the same surveys as those used in a previous manuscript published in Obesity Review in 2008. However, analysis in this paper is different and original because we use cohort analysis, i.e., we disentangle the effects of age and cohort. We show that there is a marked cohort effect in the obesity epidemic in Seychelles, i.e., BMI increased by more than 1 kg/m$^2$ per increase of 10 years of birth date of cohorts. We also show that there is no significant SES-cohort interaction, suggesting no change in social patterning of BMI over time. Moreover, by separating the effects of age and cohort, we obtain a stronger age effect. Indeed, as we mention in the discussion, in presence of a cohort effect, the observed increase in BMI along participants’ ages in a single cross sectional data would underestimate the association between BMI and age, since older persons come from older cohorts who had lower BMI at a same age. Furthermore, and more generally, as we also emphasize, cohort analysis provides an interesting way to look at data from repeat cross sectional surveys (provided surveys are population-based and use same methods for the variables of interest). This approach (cohort analysis based on serial cross sectional data) is likely to be of particularly interest in the many countries that cannot rely on systematic surveillance of the variables of interest (BMI, age, and SES in our case) to analyse data using actual cohort data. As we mention, we are not aware of any cohort analysis and related presentation of results on risk factors in the African region.

   We have added comments in the introduction of the MS and a paragraph in the discussion to make more explicit which are the advantages of both methods (direct comparison of survey data assessing a period effect, as usually done in previous studies including our study in Obesity Reviews, vs. comparison of data across different cohorts (as done in this study). We emphasize that the results are indeed consistent between the two analytic methods in the case of our data in Seychelles (but this would not have been necessarily the case had we found a non linear relation of the cohort effect).

2. The outcome of interest is BMI-measured weight status. Weight status, in part, is related to energy expenditure. Occupational physical activity is likely to contribute to energy expenditure, yet the authors combine people involved in manual and non-manual tasks into a single occupation category (i.e., the middle category).
We did not have objective measurement of physical activity (such as accelerometry, doubly labeled water), which is rarely available in population epidemiological surveys. We could therefore not disentangle underlying causes for differences in BMI across SES categories (e.g. actual energy expenditure, caloric intake, high density foods, etc).

As we explain, occupation was initially categorized in 6 categories along a standard occupation questionnaire. We further categorized SES in three categories of similar size numbers. This classification provides at least two well defined occupation categories with “labourers” (clearly manual and having usually more physical activity related to work) and “professionals” including nurses, teachers and the like (clearly non manual and being often sedentary in relation to work). Mean BMI in the intermediate occupation category fell between the first and third occupation categories. Furthermore, as we show, the distribution of these main three categories has not changed dramatically over time. We had examined in some detail the associations between different categories of SES between themselves and in relation to other behaviours in a previous report ([http://www.who.int/chp/steps/2004_STEPS_Report_Seychelles.pdf](http://www.who.int/chp/steps/2004_STEPS_Report_Seychelles.pdf)) which we now provide in reference. Of note, the three categories that we use in this study were strongly associated with behaviours that are known to be related to SES, such as alcohol drinking.

3. Does previous occupation provide an accurate representation of an unemployed person's socioeconomic status? This is further problematic for those long-term unemployed as previous occupations may have little influence on current weight status. Please also state how many participants were unemployed?

We have addressed, in the limitations section, the fact that SES in our study relied on one single occupation indicator. However, occupation is a fair indicator of SES if the employment rate is high (which is the case in Seychelles), while education is likely a less reliable indicator in developing countries experiencing major changes in education systems over the past decades. In the Seychelles, employment rate has been very high over the past decades (also due to high employment rates in government or parastatal companies as part of proactive social policy for employment by government) and was >90% in 2004 for both sexes. With regards to education, 86% of persons aged 25-34 years had completed secondary school in 2004 (there is also a very proactive government policy for education, including compulsory attendance of school for up to 11 years since more than 20 years –as long as 13 years of compulsory education 10-15 years ago with a compulsory “national youth service” at age 15-17)- vs. only 11% among persons aged 55–64 years in 1989. It is why we believe that occupation better reflects SES in countries where the education system has been changing drastically over the past 50 years.

4. Can the authors indicate the extent to which the British occupation-based Registrar Social Classes are appropriate for the Seychelles population. Do the same occupations represent the same levels of prestige or social class in the study population? Could the authors provide data about the association between occupation group and other measures of SES among the Seychelles population? This will help with ruling out other possible explanations as to why occupation might be associated with weight status (e.g., levels of occupational physical activity).

It will provide information about the strength of occupation as an SES indicator in this population.

The paper was not meant to identify reasons as to why BMI differs within SES categories but only to show whether social patterning of BMI is changing. As said above, explanations underlying social patterning of BMI would request additional data at the individual level in the 3 surveys (physical activity, nutrition, etc), which we don’t have for all three surveys.

The classification used to assess occupation has several advantages, including allowing to clearly distinguish “manual occupations” from “non manual health professionals”. This distinction is not only appropriate to distinguish persons who have expectedly different levels of physical activity related to their work (e.g. manual vs. non manual) but it also corresponds to values (prestige) in Seychelles that favour service vs. manual occupations (this is true for both sexes: there are at
least as many women as men with professional occupations, including at ministerial or parliamentary levels). This preference for “professional occupations” was already prevalent in 1989, and corresponds to values underlying fast socio-economic development in Seychelles largely along “western model”, as appearing from the ongoing close economic and cultural links with UK (from which Seychelles got independence in 1976) and Western countries. With regards to SES, we showed previously (Obesity Review), based on cross sectional data in 2004, that overweight was associated with both occupation and education and the associations were of similar strength.

5. Did an ethics committee review the study? This information is important as the Seychelles Ministry of Health funded the project. It would be good to know whether the ethics was reviewed by a committee not directly involved in the funding of the study.
Yes they were. As we mention in the text, all surveys were approved by the Ministry of Health after technical and ethical reviews, i.e. both an ethical committee and the ministry of health gave consent to each survey. Participants were free to participate and gave informed consent.

6. Please clarify what is meant by the term "examination surveys"?
This means that height and weight were measured (e.g. analogy with NHANES in USA: national health and nutrition examination surveys). However, as this may be confusing, we explicitly stated that height and weight were measured and we have replaced this word with “population-based”.

7. Who measured the respondents height and weight? Was a protocol used to undertake these measurements?
“Height was measured at 1 cm precision and weight was measured at 0.1 kg precision by trained survey officers using standard and validated weighing scales and stadiometers”. We have added this sentence in the revised MS.

8. Was the smoking question the same for all three surveys and who was considered a "current smoker"?
The questions on smoking were the same in all surveys. Cigarette smoking was defined as currently smoking at least one cigarette per day. More generally, and as stated in the manuscript, methods related to the questions of interest in this study were the same in all three surveys.

We also need you to make the following changes:

9. In the Background section of the abstract please provided an aims of the study.
We reformulated the aim of the study in the abstract and in the last sentence of the introduction.

10. You said that the study was approved by the Ministry of Health, please can you confirm in your manuscript whether this acts as an ethics committee.
As we mention in the manuscript, each survey was approved both by the ministry of health (as the survey took place within the ministry of health with personal from ministry of health) as well as by an ethical committee.