

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Cohort profile: Ho Chi Minh City Youth Cohort - changes in diet, physical activity, sedentary behavior and relationship with overweight/obesity in adolescents
<b>AUTHORS</b>	Nguyen Hoang Hanh Doan Trang, Tang Kim Hong and Michael John Dibley

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Georgia Ntani Statistician MRC LEU (Lifecourse Epidemiology Unit) University of Southampton UK  I declare no conflict of interest
<b>REVIEW RETURNED</b>	13/10/2011

<b>THE STUDY</b>	<p>The authors aimed to assess the change in nutrition status and to explore the complex associations between various factors and excess weight gain and overweight/obesity among adolescents over a period of five years in a society undergoing parallel economic changes. After well identifying the problem of childhood obesity, they consider various environmental factors as potential risk factors. The preliminary analysis that is presented here touches on the basic increasing trends of BMI across the 5 years of follow-up and the corresponding decreasing trends of time spent on MVPA/day and time spent on sedentary behaviours.</p> <p>I believe that the study is valuable, would help to investigate in greater depth one of the major health concerns and would thus interest many Public Health researchers. I have however some concerns that authors may want to take into consideration.</p> <p>COMMENT 1: The study presents the cohort profile as well as some initial analyses on the BMI trajectories, physical activity and sedentary behaviours over the 5 years of follow-up. The title however covers only one part of what is actually presented in the paper (cohort profile). I think that the preliminary analyses/findings that are also reported here should be reflected on the title of the paper, too.</p> <p>COMMENT 2: The authors provide extensive information about the multi-stage cluster cross sectional survey. Since the design of the study has been previously reported (Trang et al., 2009) and is not of direct interest for the cohort study, it seems rather confusing and misleading to get into great detail to describe it. It is absolutely respectable that the cross-sectional survey formed the basis of the cohort study and the starting point of it should be reported. My</p>
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	<p>feeling is, however, that the description of the survey should only be brief and clear (keep only essential information from 1st paragraph of sample selection) whereas the sample size of the initial survey could even be completely omitted (1st paragraph of sample size).</p> <p>COMMENT 3: It is well described in the methods how height and weight, and thus BMI, were measured. However, in the current analysis, BMI is used both as a continuous variable and as categorical (normal, overweight, obese). From the abstract I understand that IOTF age- and sex-specific cut-offs have been used to define the different categories of BMI but this should also be reported in the Methods section and the reference should also be given (Cole TJ, Bellizzi MC, Flegal KM, Dietz WH. Establishing a standard definition for child overweight and obesity worldwide: international survey. <i>BMJ</i> 2000;320:1240–3).</p> <p>COMMENT 4: In the Methods/Sample Selection the authors describe that 11 schools were from wealthy districts and 7 were from less wealthy districts. It would be useful for the reader to be clarified what exactly the authors mean by 'wealthy' - 'less wealthy'. How were these categories defined?</p> <p>COMMENT 5: It is suggested that the authors add in the methods section what expected differences in BMI were considered between exposure groups when estimating the sample size.</p> <p>COMMENT 6: Table 2 provides very useful information on the baseline characteristics of the cohort. My query is how the socioeconomic status has been defined. My understanding is that socioeconomic status was expressed as a score which was then divided into thirds to create the categorisation presented in Table 2 (from poorest to richest). How has the score been created? Is that an affluence score? I would suggest the authors to give more details on that or give a reference in the Methods section.</p> <p>COMMENT 7: Another important concern is the potential selection effect of the school type. The authors have not given details of the number of children in the cohort study attending public and private schools questioning the representativeness of the sample. Also, if the vast majority of the participants attend public schools then associations explored in future research on this cohort may be biased and that should be reported as a limitation in the study.</p> <p>COMMENT 8: I am bit confused over the estimated sample size. The authors end up with a number of 720 children that need to be recruited in the study and then they report that this represents 60% of urban junior high school students in the cross-sectional survey who were invited to participate in the cohort study. Aren't they 784 students that were invited?</p> <p>COMMENT 9: In the statistical analysis part of the methods the authors mention that baseline characteristics were compared using Pearson's chi-square or Fisher's exact test for categorical data and t-test and Mann-Whitney test for continuous data. However, no comparison test is presented in Table 1 for the baseline data nor it is reported anywhere in the text. I would suggest either to add a column in Table 1 to report the comparison between boys and girls or to remove that part from data analysis part. My other concern regarding the statistical analysis is that while the authors report a test for trend by years of follow-up they don't do the same for time</p>
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	<p>spent on MVPA and sedentary behaviours. It would be of interest to also report the trend in the median time of MVPA and sedentary behaviours across the years of follow-up.</p> <p>COMMENT 10: Table 3 presents the prevalence of overweight and obesity for each year of follow-up. The prevalence of overweight increases from 12.5 to 18.3 which could be expressed as a 46.4% increase or a 9.3% increase per year of follow-up. Similarly, obesity increases from 1.7% to 6.2% which is a 52.9% increase per year of follow-up. However, the corresponding figures in the results section do not match. Could the authors clarify how they came up with the figures reported (p.13 line 44)? The same for the figures from the reference 21 (Julia M et al, 2008)</p> <p>COMMENT 11: The authors do not comment on possible limitations of the study that are likely to affect future analysis. The relatively small sample size could be mentioned as a basic weakness.</p> <p>COMMENT 12: Another minor and easily correctable point has to do with the measured distance from schools to participants' homes. The distance is described by the use of mean but the interquartile range is given in brackets. Is distance normally distributed? If yes, standard deviation should better be reported instead of IQR and if it's skewed the median would better describe the distribution of the variable.</p> <p>COMMENT 13: Minor. Report in the Abstract the number of children in the cohort that were followed-up over five years.</p> <p>COMMENT 14: Minor. The standard of English is high and perfectly acceptable for publication. I have however spotted a couple of grammatical errors and typos which I list out below to help the authors:  p.3 1st bullet point on strengths and limitations: long hard to follow sentence  p.4 line 14: delete "in 2002" and "in 2004" at the end of the sentence. The two years are mentioned earlier in previous line and later the sentence reads "over the same period"  p.11 lines 3-11: Means, standard deviations, medians, interquartile ranges, confidence intervals instead of mean, standard deviation, median, range, confidence interval.  p.11 line 17: "...by gender and the lost to follow-up group was compared with..."  p.12 line 15: In the second round of follow-up, there were 712 participants (not 740)</p>
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<b>REVIEWER</b>	<p>Do TP Ha  Head of Community Nutrition Department  National Institute of Nutrition  Vietnam</p> <p>I declare to have no competing interest.</p>
<b>REVIEW RETURNED</b>	17/11/2011

<b>RESULTS &amp; CONCLUSIONS</b>	<p>The discussion seems insufficient. The authors need to work more on discussion on the observed changes of each outcomes in relation with previous evidence in Vietnam and other countries, their possible causes, consequences and solutions.</p>
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## VERSION 1 – AUTHOR RESPONSE

Reviewer: Georgia Ntani  
Statistician  
MRC LEU (Lifecourse Epidemiology Unit)  
University of Southampton  
UK

I declare no conflict of interest

The authors aimed to assess the change in nutrition status and to explore the complex associations between various factors and excess weight gain and overweight/obesity among adolescents over a period of five years in a society undergoing parallel economic changes. After well identifying the problem of childhood obesity, they consider various environmental factors as potential risk factors. The preliminary analysis that is presented here touches on the basic increasing trends of BMI across the 5 years of follow-up and the corresponding decreasing trends of time spent on MVPA/day and time spent on sedentary behaviours.

I believe that the study is valuable, would help to investigate in greater depth one of the major health concerns and would thus interest many Public Health researchers. I have however some concerns that authors may want to take into consideration.

COMMENT 1: The study presents the cohort profile as well as some initial analyses on the BMI trajectories, physical activity and sedentary behaviours over the 5 years of follow-up. The title however covers only one part of what is actually presented in the paper (cohort profile). I think that the preliminary analyses/findings that are also reported here should be reflected on the title of the paper, too.

Thanks for your comments. We have changed the title as suggested:

Cohort profile: Ho Chi Minh City Youth Cohort - changes in diet, physical activity, sedentary behavior and relationship with overweight/obesity and metabolic syndrome in adolescents

COMMENT 2: The authors provide extensive information about the multi-stage cluster cross sectional survey. Since the design of the study has been previously reported (Trang et al., 2009) and is not of direct interest for the cohort study, it seems rather confusing and misleading to get into great detail to describe it. It is absolutely respectable that the cross-sectional survey formed the basis of the cohort study and the starting point of it should be reported. My feeling is, however, that the description of the survey should only be brief and clear (keep only essential information from 1st paragraph of sample selection) whereas the sample size of the initial survey could even be completely omitted (1st paragraph of sample size).

Thank you for your suggestion. The details of the sample size of the initial cross sectional survey have been removed

COMMENT 3: It is well described in the methods how height and weight, and thus BMI, were measured. However, in the current analysis, BMI is used both as a continuous variable and as categorical (normal, overweight, obese). From the abstract I understand that IOTF age- and sex-specific cut-offs have been used to define the different categories of BMI but this should also be reported in the Methods section and the reference should also be given (Cole TJ, Bellizzi MC, Flegal KM, Dietz WH. Establishing a standard definition for child overweight and obesity worldwide: international survey. *BMJ* 2000;320:1240–3).

Thank you for your suggestion. This information has now been added in the data analysis part-Method section as follows:

The IOTF (International Obesity Task Force) BMI cut-off values were used to define overweight and obesity combined [24].

COMMENT 4: In the Methods/Sample Selection the authors describe that 11 schools were from wealthy districts and 7 were from less wealthy districts. It would be useful for the reader to be clarified what exactly the authors mean by 'wealthy' - 'less wealthy'. How were these categories defined? Thank you for your suggestion. This information was added in the sample selection part- Method section as follows:

This classification of urban districts was derived from the Statistics Review of HCMC province Department of Statistics.[10]

COMMENT 5: It is suggested that the authors add in the methods section what expected differences in BMI were considered between exposure groups when estimating the sample size.

The following paragraph was added in the methods section for further clarification:

In the absence of information about relative change in BMI for HCMC children, these sample size estimates are based on the observed relative change in BMI in children exposed to an education intervention to reduce TV viewing in the United States.[11] The average change in BMI selected was 0.35 kg/m<sup>2</sup>.

COMMENT 6: Table 2 provides very useful information on the baseline characteristics of the cohort. My query is how the socioeconomic status has been defined. My understanding is that socioeconomic status was expressed as a score which was then divided into thirds to create the categorisation presented in Table 2 (from poorest to richest). How has the score been created? Is that an affluence score? I would suggest the authors to give more details on that or give a reference in the Methods section.

Thank you for your suggestion. This information was added in the Method section as follows:

To assess economic status, ownership of an inventory of assets was used to construct a household wealth index using the principal components method to assign a weight for each asset. [22] A total of fourteen assets were assessed including bicycles, motorbikes, televisions, radios, videos, cassette players, computers, gas stoves, CD players, cars, microwave ovens, refrigerator and telephone and air-conditioners. The basis for selecting the assets was from a report of the Bureau of Statistics of HCMC [10] listing the most common assets used among HCMC population.

And in the data analysis part:

The wealth index was ranked and divided into tertiles and each household was assigned to one of these wealth index categories

COMMENT 7: Another important concern is the potential selection effect of the school type. The authors have not given details of the number of children in the cohort study attending public and private schools questioning the representativeness of the sample. Also, if the vast majority of the participants attend public schools then associations explored in future research on this cohort may be biased and that should be reported as a limitation in the study.

Many thanks to the reviewer for pointing this out, a paragraph has now been added to the discussion as follows:

Although there was only one non-public school in this study, this reflected the small proportion of the school population enrolled in this type of school in HCMC. We found no important differences between public and private schools for the main study factors and outcomes, such as socioeconomic status. Unlike the situation in many developed countries, in Vietnam, non-public schools are not popular and students usually enter these schools when they cannot secure a place in a public school. Thus, we believe the sampling of this study was not biased and is representative of HCMC adolescents at school.

And in the methods – sample selection part:

Because the number of classes and students in non-public schools are smaller than in public schools, only 3 non-public schools were selected in the sample of the cross-sectional survey.

COMMENT 8: I am bit confused over the estimated sample size. The authors end up with a number of 720 children that need to be recruited in the study and then they report that this represents 60% of urban junior high school students in the cross-sectional survey who were invited to participate in the

cohort study. Aren't they 784 students that were invited?

This part was restructured as follows:

However, since we used cluster sampling (school and class selection), all the students in a selected class were invited to participate in the cohort for logistic reasons; yielding 784 students who were invited to participate and giving 759 who were recruited into the cohort study.

COMMENT 9: In the statistical analysis part of the methods the authors mention that baseline characteristics were compared using Pearson's chi-square or Fisher's exact test for categorical data and t-test and Mann-Whitney test for continuous data. However, no comparison test is presented in Table 1 for the baseline data nor it is reported anywhere in the text. I would suggest either to add a column in Table 1 to report the comparison between boys and girls or to remove that part from data analysis part. My other concern regarding the statistical analysis is that while the authors report a test for trend by years of follow-up they don't do the same for time spent on MVPA and sedentary behaviours. It would be of interest to also report the trend in the median time of MVPA and sedentary behaviours across the years of follow-up.

Thank you for your comments. The p value to report the comparison between boys and girls has been added to table 1.

A test for change by years has been presented for the time in MVPA and sedentary behaviors.

COMMENT 10: Table 3 presents the prevalence of overweight and obesity for each year of follow-up. The prevalence of overweight increases from 12.5 to 18.3 which could be expressed as a 46.4% increase or a 9.3% increase per year of follow-up. Similarly, obesity increases from 1.7% to 6.2% which is a 52.9% increase per year of follow-up. However, the corresponding figures in the results section do not match. Could the authors clarify how they came up with the figures reported (p.13 line 44)? The same for the figures from the reference 21 (Julia M et al, 2008)

We have restructured this paragraph as follows:

In Ho Chi Minh City over the five year period, the prevalence of overweight increased from 12.5% to 16.7% and obesity increased from 1.7% to 5.1%. This increase in overweight and obesity was consistent with the increases found in 5-year follow up studies from 1999 to 2004 in urban Indonesian children where the prevalence of overweight increased from 4.2% in children to 8.8% in adolescents, and similarly obesity increased from 1.9% to 3.2% [28].

COMMENT 11: The authors do not comment on possible limitations of the study that are likely to affect future analysis. The relatively small sample size could be mentioned as a basic weakness.

Thanks for your suggestion. This information was added for more clarification

The relatively small sample size is one limitation of the study restricting examination of all potential relationships because of the limited sample in some subgroups. However, the sample size was adequate to detect changes in BMI over time and changes in key study factors such as physical activity and screen time which was used as a proxy for sedentary time. Despite this restriction, this study remains important because it is one of very few longitudinal studies in Vietnam and South East Asia to explore obesity and changes in obesity-related risk factors in adolescents.

COMMENT 12: Another minor and easily correctable point has to do with the measured distance from schools to participants' homes. The distance is described by the use of mean but the interquartile range is given in brackets. Is distance normally distributed? If yes, standard deviation should better be reported instead of IQR and if it's skewed the median would better describe the distribution of the variable.

Thank you for your suggestion. Since the distance is not normally distributed, the median value and IQR were presented.

COMMENT 13: Minor. Report in the Abstract the number of children in the cohort that were followed-up over five years.

Thank you for your suggestion. This information was added to the abstract

At baseline, 759 students were recruited into the cohort, and out of these students, 740 remained in the cohort for the 1st round of follow-up, 712 for the 2nd round, 630 for the 3rd round, and 585 students for the last follow-up.

COMMENT 14: Minor. The standard of English is high and perfectly acceptable for publication. I have however spotted a couple of grammatical errors and typos which I list out below to help the authors:  
 p.3 1st bullet point on strengths and limitations: long hard to follow sentence  
 p.4 line 14: delete “in 2002” and “in 2004” at the end of the sentence. The two years are mentioned earlier in previous line and later the sentence reads “over the same period”  
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 p.12 line 15: In the second round of follow-up, there were 712 participants (not 740)  
 Thank you for your suggestions. We have corrected these mistakes.

Reviewer: Do TP Ha  
 Head of Community Nutrition Department  
 National Institute of Nutrition  
 Vietnam

I declare to have no competing interest.

The discussion seems insufficient. The authors need to work more on discussion on the observed changes of each outcomes in relation with previous evidence in Vietnam and other countries, their possible causes, consequences and solutions.

Thank you for your comments. In this paper, we focus on the cohort profile, not the details of each of the outcomes measured in the study as these will be the subject of detailed analyses in future papers. However, a paragraph was added at the end of the results to explicitly mention these papers: Previous papers based on the cross sectional baseline survey have described overweight and obesity [3 25], physical inactivity [21], and metabolic syndrome [26] and associated risk factors. Another paper has used the longitudinal data from the youth cohort to describe changes in active commuting to school and risk factors for changes in commuting status [27]. More results will be presented in future papers presenting changes in physical activity, sedentary behaviour, and overweight/obesity and their associated risks factors.

And we have added paragraphs in the discussion correspondingly

In Ho Chi Minh City over the five year period, the prevalence of overweight increased from 12.5% to 16.7% and obesity increased from 1.7% to 5.1%. This increase in overweight and obesity was consistent with the increases found in 5-year follow up studies from 1999 to 2004 in urban Indonesian children where the prevalence of overweight increased from 4.2% in children to 8.8% in adolescents, and similarly obesity increased from 1.9% to 3.2% [28]. Similar findings have been reported from a 5-year follow-up study in Thai school children where the prevalence of overweight in boys increased from 12.4% in 1992 to 21.0% in 1997.[29]

The primary findings of this study are also consistent with the substantial age-related declines in MVPA among both adolescent boys and girls previously reported in the US [30-31], and Finland [32]. These results also showed that daily MVPA decreased of 42% over a five year period when adolescents were aged 12 years to 16 years, which was similar to the 45% decline reported for American adolescents in the CATCH study [33]. Furthermore, the increase in sedentary time with age found in the present study is similar to increases reported by longitudinal studies in developed countries [34-35].

**VERSION 2 – REVIEW**

<b>REVIEWER</b>	Georgia Ntani
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	<p>Statistician MRC LEU (Lifecourse Epidemiology Unit) University of Southampton UK</p> <p>I declare no conflict of interest</p>
<b>REVIEW RETURNED</b>	12/12/2011

<b>THE STUDY</b>	<p>The authors have successfully addressed most of the suggested comments resulting in a clearly improved paper. However, before moving further, I would like to raise the issue of differences in the revised paper that were not suggested by any reviewer to my knowledge and were not discussed by the authors in their response either. I am particularly referring to the figures reported in table 3 and specifically for the variables of BMI and time spent on sedentary behaviours. I understand that in the un-revised paper, data from the V-APARQ were used whereas in the current (revised) paper, data from the ASAQ were used. The previous variable gave no statistical significance of time spent on sedentary behaviours whereas the current variable presents a significant trend across time. Could the authors please explain i) why they have decided to change the variable presented and thus presenting a different association (significant change in sedentary behaviours) than what they presented in the first round of the review and ii) why the prevalence of overweight and obesity have changed?</p> <p>I am grateful that the authors took into consideration the comments suggested. However, I am still a bit concerned about a couple of things that I list out below:</p> <p>COMMENT 1: The revised title of the paper reads “[...] changes in diet, physical activity, sedentary behaviour and relationship with overweight/obesity and metabolic syndrome in adolescents”. Even though, the revised title is more descriptive now, I think that the words diet and metabolic syndrome should be dropped from the title since no change in diet nor metabolic syndrome has been explored in the current analysis.</p> <p>COMMENT 2: I would like to thank the authors for reference that they give in response to my comment on the classification of urban districts. However, since the reference is not readily and widely accessible could the authors briefly describe (in maximum of a couple of sentences) how ‘wealthy’ and ‘non-wealthy’ were defined?</p>
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**VERSION 2 – AUTHOR RESPONSE**

1. Reviewer: Georgia Ntani  
Statistician  
MRC LEU (Lifecourse Epidemiology Unit)  
University of Southampton  
UK

I declare no conflict of interest



R1.1. The authors have successfully addressed most of the suggested comments resulting in a clearly improved paper. However, before moving further, I would like to raise the issue of differences in the revised paper that were not suggested by any reviewer to my knowledge and were not discussed by the authors in their response either. I am particularly referring to the figures reported in table 3 and specifically for the variables of BMI and time spent on sedentary behaviours. I understand that in the un-revised paper, data from the V-APARQ were used whereas in the current (revised) paper, data from the ASAQ were used. The previous variable gave no statistical significance of time spent on sedentary behaviours whereas the current variable presents a significant trend across time. Could the authors please explain i) why they have decided to change the variable presented and thus presenting a different association (significant change in sedentary behaviours) than what they presented in the first round of the review and ii) why the prevalence of overweight and obesity have changed?

A.1.1. Thank you for your comments and we are so sorry for not having highlighted these changes in the previous responses.

As mentioned in the methods that in the third round, we collected biochemical data for assessing cardiovascular and metabolic disease risks. In the previous version, we used data from adolescents having both blood test and anthropometric data in this third round (n=617) instead of the whole observations (n=630), resulting in the incorrect presentation of measurement of BMI status and sedentary time for the whole sample from this survey year onwards. In this version, we correctly presented the findings from the whole sample. For the sedentary data, the previous p-value was the typo mistake. It was 0.001, not 0.4.

The BMI and sedentary time data corrected were not significantly different from the data among those having both anthropometric and blood test results in the previous version. Giving the same statistical significance of the values, we haven't considered it the major correction that may influence the importance of our study.

Furthermore, we have renamed the questionnaires to be more precisely described the questionnaires that we used for physical activity (V-APARQ) and for sedentary behavior (ASAQ). The ASAQ was used to describe correctly time spent for each day of the week in a range of sedentary activities including watching television/video; playing computer games; using computer for fun; using computer for study; studying at home; studying in after-school class; reading books; chatting with friends; talking on phone; doing other sedentary hobbies; music or painting lesson/practice; passive commuting to school (car, bus, motorbike). Daily time spent in sedentary behavior was computed as the total of all recorded sedentary activities.

R1.2. I am grateful that the authors took into consideration the comments suggested. However, I am still a bit concerned about a couple of things that I list out below:

COMMENT 1: The revised title of the paper reads "[...] changes in diet, physical activity, sedentary behaviour and relationship with overweight/obesity and metabolic syndrome in adolescents". Even though, the revised title is more descriptive now, I think that the words diet and metabolic syndrome should be dropped from the title since no change in diet nor metabolic syndrome has been explored in the current analysis.

A1.2. Thank you for your helpful suggestion. We would drop the "metabolic syndrome" here but we prefer to keep the "diet" because we aim to present the cohort profile of the study in which we explore the change of physical activity, sedentary behavior and diet and their relationship with overweight/obesity even though we haven't explored the diet change in this analysis yet. The title will then be:

Cohort profile: Ho Chi Minh City Youth Cohort - changes in diet, physical activity, sedentary behavior and relationship with overweight/obesity in adolescents

R1.3. COMMENT 2: I would like to thank the authors for reference that they give in response to my comment on the classification of urban districts. However, since the reference is not readily and widely accessible could the authors briefly describe (in maximum of a couple of sentences) how 'wealthy' and 'non-wealthy' were defined?

A1.3. Thank you for your comments. We have added a paragraph with more details as follows: Non-wealthy districts were categorized as having more than 50% households below the wealthy threshold (non-wealthy household). Non-wealthy household in Ho Chi Minh City in 2004 was identified as having average personal income less than 6.000.000 Vietnam Dong/person/year.[11]

### VERSION 3 - REVIEW

<b>REVIEWER</b>	Georgia Ntani Statistician MRC Lifecourse Epidemiology Unit University of Southampton Southampton UK  I declare no conflict of interest
<b>REVIEW RETURNED</b>	24/01/2012

<b>THE STUDY</b>	The authors have addressed all my comments and have covered my queries. I am grateful that they took into consideration my comment in changing the original title of the paper. I am still surprised though that they insist on keeping the word "diet" in the title even if no analysis results have been presented on diet in the current paper. However, I feel that this is an editorial decision.  I am happy for the paper to be published as it is a very well written manuscript on a truly interesting topic.
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