PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Prospective Cohort Study of Overweight and Obesity among Rural Indian Adults: Socio-demographic Predictors of Prevalence, Incidence, and Remission
AUTHORS	Rai, Rajesh Kumar; Jaacks, Lindsay; Bromage, Sabri; Barik, Anamitra; Fawzi, Wafaie.; Chowdhury, Abhijit

VERSION 1 – REVIEW

REVIEWER	Dr Madhavi Bhargava Yenepoya Medical College, Mangalore, India.
REVIEW RETURNED	06-Mar-2018

GENERAL COMMENTS	Overweight and Obesity among Rural Indian Adults: Socio-demographic and Behavioural Predictors of Prevalence, Incidence, and Remission. Authors develop a paper based on Health and Demographic Surveillance System data of 2008 and 2017 to evaluate predictors of prevalence, incidence and remission of overweight and obesity in rural adults. In general, it is well written article and an important one. Following are some suggestions and corrections: Title: The term behavioural predictors of overweight and obesity is misleading. The only behavioural factors considered are smoking and alcohol. As authors declare in the manuscript itself that more proximal behavioural factors like diet and physical activity are not studied, I would suggest that this be not included in the title. They have a fairly large data set with many important socio-demographic predictors and that is it Abstract: The inclusion of the statement, 'smokeless tobacco use was negatively associated with overweight in 2008 among both groups' is unnecessary. Since the analysis does not include the underweight individuals this is misleading for a reader who may stop at the abstract. Instead, I suggest a statement about remission will make the abstract interesting. Remission is not often studied and should be highlighted in the abstract. Introduction: Line no: 98-99 about federal budgeting in India for NCD needs a reference which is separate from the reference for plateaued development assistance from international agencies. In a way, it is a redundant statement as the article does not touch on this aspect at all! Methods: 2176 died from 2008 to 2017 according to line 141. It will be interesting to know the nutrition characteristics of these persons.

	Since the authors declare that no additional data are available, this important and relevant information will not be known. It is not the objective of the article, but mortality and nutrition have known associations. Results: The baseline characteristics should have included the height and weight data, more-so because authors have included the Asian cut-offs. Poor heights are known in Indians and that information is relevant. This becomes important because authors declare that no additional data are available. Discussion: Education of 6+ leading to sedentary lifestyle is very unlikely, given the number of educated unemployed in India and West Bengal. It needs to be discussed appropriately along side of other predictors. The possibility of Hindus being more likely to be vegetarians is unlikely especially in West Bengal where the SRS data shows high prevalence of non-vegetarian food habit. Since this information could have been collected in terms of primary data and has not been done, it best not generalized. Authors discuss that smokeless tobacco users are more likely to be underweight. But this does not mean that it is protective for overweight/obesity and needs greater clarification in the discussion and removal from the abstract. Tables: There is a small mistake in Table 1: the header row mentions overweight in 2008. This almost gives the impression that the 'n' of men and women in the row below it indicates overweight numbers which is not so. General remark: Authors are discussing 'nutrition transition' but looking at their paper (Reference 19) on underweight in the same population, I think what we see is the presence of significant double burden of disease. The separation of both nutritional states in both these papers prevents a reader from getting a wholesome picture. Since the underweight paper follows up the cohort only till 2012, it would have been preferable if there was inclusion of both ends of the spectrum for a complete picture in this article. According to WHO, a population prevalence of low BMI above 40% is considered a 'critical' situation, and that is the case with this population too. It was 46% to begin with in 2008 and 43% in 2017.

REVIEWER	Tabassum Nawab Department of Community Medicine, J.N. Medical College, Faculty of Medicine, Aligarh Muslim University, Aligarh, India
REVIEW RETURNED	10-Mar-2018

GENERAL COMMENTS	Study limitations should also include lack of current socio-demographic data and behavioural data in 2017- like marital status, highest level of education attained, employment status, wealth quintile, and also smoking and alcohol intake, use of smoke-less tobacco- which might have changed in course of 9 years and may show different results for 'predictors of overweight in 2017'. It might have changed the conclusion and recommendations also. Other limitations including lack of important behavioural predictors have been mentioned by the authors

VERSION 1 – AUTHOR RESPONSE

Reviewer # 1
Comment: Title: The term behavioural predictors of overweight and obesity is misleading. The only behavioural factors considered are smoking and alcohol. As authors declare in the manuscript itself that more proximal behavioural factors like diet and physical activity are not studied, I would suggest that this be not included in the title. They have a fairly large data set with many important socio-demographic predictors and that is it.
Response: We agree with the reviewer and have revised the title of the manuscript to “Prospective Cohort Study of Overweight and Obesity among Rural Indian Adults: Socio-demographic Predictors of Prevalence, Incidence, and Remission.”

Comment: Abstract: The inclusion of the statement, ‘smokeless tobacco use was negatively associated with overweight in 2008 among both groups’ is unnecessary. Since the analysis does not include the underweight individuals this is misleading for a reader who may stop at the abstract. Instead, I suggest a statement about remission will make the abstract interesting. Remission is not often studied and should be highlighted in the abstract.
Response: Thank you for this thoughtful comment – we 100% agree with the reviewer that a novel aspect of this study is the inclusion of obesity remission. To address this comment, we have removed the description from the abstract. In its place, we have added our interesting finding relating to remission of overweight varying by administrative block of residence.

Comment: Introduction: Line no: 98-99 about federal budgeting in India for NCD needs a reference which is separate from the reference for plateaued development assistance from international agencies. In a way, it is a redundant statement as the article does not touch on this aspect at all!
Response: The reviewer makes a great point regarding the redundancy of the statement regarding plateauing of development assistance from international agencies. We have therefore removed that statement, retaining the statement regarding negligible federal funding to address obesity in India and adding an appropriate reference (Menon et al. Matern Child Nutr 2016; 12 Suppl 1:169-85). We feel that this statement is important to add some context for the global readership of BMJ Open and demonstrate the severity of the situation and lack of response of the Indian government to the overweight and obesity epidemic.

Comment: Methods: 2176 died from 2008 to 2017 according to line 141. It will be interesting to know the nutrition characteristics of these persons. Since the authors declare that no additional data are available, this important and relevant information will not be known. It is not the objective of the article, but mortality and nutrition have known associations.
Response: Thank you for raising this interesting point. Unfortunately, as the reviewer points out, we do not have data on the nutritional status of those who died.

Comment: Results: The baseline characteristics should have included the height and weight data, more-so because authors have included the Asian cut-offs. Poor heights are known in Indians and that information is relevant. This becomes important because authors declare that no additional data are available.
Response: We have added the gender-stratified height and weight summary statistics in Table 1.

Comment: Discussion: Education of 6+ leading to sedentary lifestyle is very unlikely, given the number of educated unemployed in India and West Bengal. It needs to be discussed appropriately along side of other predictors.
The possibility of Hindus being more likely to be vegetarians is unlikely especially in West Bengal where the SRS data shows high prevalence of non-vegetarian food habit. Since this information could have been collected in terms of primary data and has not been done, it best not generalized.

Authors discuss that smokeless tobacco users are more likely to be underweight. But this does not mean that it is protective for overweight/obesity and needs greater clarification in the discussion and removal from the abstract.
Response: Thank you for your advice on education, religion, and smoking parameters. As you have advised, we have added clarification regarding interpretation of education of 6+ leading to sedentary lifestyle. We believe that in rural West Bengal, where the study was conducted, some amount of education seems to make a difference. In revised text, we have clarified that the interpretation of the finding should be made with some caution.
Regarding the reviewer's comment on vegetarianism, we agree that an important limitation of this study is the lack of data on dietary habits. Nonetheless, we observed that individuals reporting Muslim religion in this population were significantly more likely to be overweight or obese compared to individuals reporting Hindu religion. In the discussion, we proposed one hypothesis for this (potentially higher consumption of meat among Muslim people compared to Hindu people – and meat consumption is associated with obesity in other prospective cohort studies). We have revised the text to emphasize that this is just one potential hypothesis, and further research is needed.
Regarding smoking, we have added a sentence stating that the finding should be interpreted with caution as this article does not suggest that smoking is a protection factor for either underweight or overweight. As you have advised, we have removed the result from the abstract.

Comment: Tables: There is a small mistake in Table 1: the header row mentions overweight in 2008. This almost gives the impression that the 'n' of men and women in the row below it indicates overweight numbers which is not so.
Response: Thank you very much. It was our mistake. We have corrected the table.

Comment: General remark: Authors are discussing 'nutrition transition' but looking at their paper (Reference 19) on underweight in the same population, I think what we see is the presence of significant double burden of disease. The separation of both nutritional states in both these papers prevents a reader from getting a wholesome picture. Since the underweight paper follows up the cohort only till 2012, it would have been preferable if there was inclusion of both ends of the spectrum for a complete picture in this article. According to WHO, a population prevalence of low BMI above 40% is considered a 'critical' situation, and that is the case with this population too. It was 46% to begin with in 2008 and 43% in 2017.
Response: The reviewer's comment is spot-on. These data do indeed demonstrate a significant double burden of under- and over-nutrition in this rural district of West Bengal, India. This is consistent with the recent nutrition transition literature, which suggests that the very rapid and unequal economic development occurringin low- and middle-income countries has resulted in overweight and obesity, largely in women and individuals with high socio-economic status, whilst underweight persists in poorer individuals. Eventually, as has already been witnessed in some middle-income countries such as China, Brazil, and Thailand, the burden of overweight and obesity is likely to shift to those with lower socio-economic status. Importantly, our data suggest that this is not yet the case in India. We heartily agree with the reviewer that we cannot interpret these data on overweight and obesity prevalence, incidence, and remission in a vacuum. We also recognize that it would be outside the scope of this study to re-analyze all of the predictors of underweight prevalence, incidence, and remission – and that these predictors are unlikely to have changed since 2012. However, we do include the updated underweight prevalence, incidence, and remission numbers in Figure 1 for 2017, directly next to the numbers for overweight and obesity, and have revised the text throughout to view our findings on overweight and obesity through the lens of the full malnutrition spectrum. Thank you again for this very useful comment!

Reviewer # 2
Comment: Study limitations should also include lack of current socio-demographic data and behavioural data in 2017- like marital status, highest level of education attained, employment status,

wealth quintile, and also smoking and alcohol intake, use of smoke-less tobacco- which might have changed in course of 9 years and may show different results for 'predictors of overweight in 2017'. It might have changed the conclusion and recommendations also. Other limitations including lack of important behavioural predictors have been mentioned by the authors.
Response: Thank you for pointing this out. You may have noticed the online supplement Table 2, where we have provided analysis of predictors of overweight in 2017. On page 11, we have added the description of it as well, highlighting how the findings vary from predictors of overweight in 2008. Finally, we have added to the limitations that we did not have updated socio-demographic data for participants in 2017.

VERSION 2 – REVIEW

REVIEWER	Madhavi Bhargava Yenepoya Medical College, Center for Nutrition Studies, Yenepoya (Deemed to be University) Mangalore, India
REVIEW RETURNED	29-Apr-2018

GENERAL COMMENTS	All the suggestions made in the previous review have been adequately addressed. No further revision required from my side.

REVIEWER	Tabassum Nawab, Assistant Professor Department of Community Medicine, Faculty of Medicine, Aligarh Muslim University, Aligarh, India
REVIEW RETURNED	12-May-2018

GENERAL COMMENTS	In the online supplement Table 2 Predictors of overweight in 2017, only data for age should be shown, as it is updated for 2017. Rest of socio-demographic variables which have not been updated should be omitted. it is very misleading to show the old data as predictors of overweight after a gap of 9 years. also the paper can do justice to the aims and objective even without this table. Already we have allowed the baseline socio-demographic characteristics to be considered as predictors for incidence and remission of overweight in 2017(which is a limitation, but allowable).

VERSION 2 – AUTHOR RESPONSE

Reviewer # 1

Comment: All the suggestions made in the previous review have been adequately addressed. No further revision required from my side.

Response: Thank you for the approval for publication.

Reviewer # 2

Comment: In the online supplement Table 2 Predictors of overweight in 2017, only data for age should be shown, as it is updated for 2017. Rest of socio-demographic variables which have not been updated should be omitted. it is very misleading to show the old data as predictors of overweight after a gap of 9 years. also the paper can do justice to the aims and objective even without this table.

Already we have allowed the baseline socio-demographic characteristics to be considered as predictors for incidence and remission of overweight in 2017(which is a limitation, but allowable).

Response: Thank you. We have made changes as advised, in online supplement Table 2.