

## 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

<b>TITLE (PROVISIONAL)</b>	Correlates of the double burden of malnutrition among women: an analysis of cross sectional survey data from sub-Saharan Africa
<b>AUTHORS</b>	Amugsi, Dickson; DIMBUENE, ZACHARIE; Kyobutungi, Catherine

## VERSION 1 - REVIEW

<b>REVIEWER</b>	Dr Rishi Caleyachetty University of Warwick, UK
<b>REVIEW RETURNED</b>	10-Feb-2019

<b>GENERAL COMMENTS</b>	<p>While the double burden of malnutrition in LMICs is an important topic, the use of this work to inform practice or policy is quite limited.</p> <p>The major concerns are:</p> <ol style="list-style-type: none"><li>1) Part of this topic has been covered before:  Neupane S, Prakash KC, Doku DT. Overweight and obesity among women: analysis of demographic and health survey data from 32 Sub-Saharan African Countries. BMC Public Health. 2016 Jan 13;16:30</li><li>2) Why did the authors only select five SSA countries? There is no explanation how countries are selected.</li><li>3) The authors use a limited concept of double burden of malnutrition. There is no consideration of stunting and micro-nutrient deficiency.</li><li>4) There is no justification for choice of predictors. For e.g. why is frequency of watching tv or marital status associated with thinness or overweight/obesity?</li><li>5) Many tables with little consideration on how to appropriately present the results. For e.g. are both 95% CI and p-values needed?</li></ol> <p>Minor comment:</p> <p>There is unnecessary detail on equations which is unlikely to be appreciated by readers of the journal.</p>
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<b>REVIEWER</b>	Tiffany Gill The University of Adelaide, Australia
<b>REVIEW RETURNED</b>	22-Feb-2019

<b>GENERAL COMMENTS</b>	<p>Thank you for the opportunity to review this paper. My comments are as follows:</p> <p>Abstract: Appropriate.</p> <p>Introduction: Last line, first paragraph, should be "increases the risk of death"  Second paragraph, page 5, line 10 should be "being overweight as a mother.."  line 13, "long-term weight excess.." should be "long-term excessive weight."  Introduction is appropriate and clear.</p> <p>Methodology: Page 9 line 21, multivariate should be multivariable  The mathematical description of multinomial logistic regression could be included as supplementary information.  The predictor variables of interest are described on page 9. It is then suggested in the last paragraph of the methods that there were no candidate predictor variables.  Perhaps it should just be stated that all predictors were entered into the model.  Sampling is well described and it is noted that the data are standardized to allow cross country comparison but what are the data standardized to?  There is no statement regarding ethics or consent for these studies and this should be added.</p> <p>Results: Descriptive section, line 21, should be "Women in Kenya.."  Line 24 "fairly the same" should be "similar"  Multivariate section, page 13, should be Multivariable. This should also be changed throughout.</p> <p>Tables are appropriate.</p> <p>Discussion: Has SSA been defined in the text?  Page 22 line 3, should "dietary patterns" be "poor dietary patterns"  Line 6 "Contrariwise" should be "In contrast"  Line 15 "associate" should be "associated"  Some other issues with English impact on the clarity with the discussion.</p> <p>Conclusion: Appropriate, however stating "old age" is significantly associated is somewhat problematic. The age range (15-49) in this study may be "old age" however may not be "old age" for the reader. It would be useful to put the age range used in this study into context of life expectancy for these countries. This perhaps could be done in the background.</p>
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<b>REVIEWER</b>	Carla Alberici Pastore Federal University of Pelotas - Brazil
<b>REVIEW RETURNED</b>	28-Mar-2019

<b>GENERAL COMMENTS</b>	<p>Congrats to the authors, it is a very interesting and well writing manuscript.</p> <p>I have only a few sugestions to point, in the better intention of improve the manuscript:</p> <ol style="list-style-type: none"> <li>1. The WHO BMI classification used in the study was designed for adult people (18 years old and over). But the sample contains women between 15 and 18 years old. For those, WHO suggests to use the Growth Standards Curves, which allows to adolescents to present lower BMI without being underweighted. So, the use of adult's BMI cuttpoints could had overestimated the prevalence of undernutrition in this subsample of under 18 years old women.</li> <li>2. On table 2, I suggest to include the Standard Deviation (DP), and not the Standard Error, along with the mean values (in continuous variables).</li> </ol> <p>Still on table 2, the term "Currently breastfeeding" in the first column should be in bolded font.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Dr Rishi Caleyachetty

Institution and Country: University of Warwick, UK

Please state any competing interests or state 'None declared': None

Please leave your comments for the authors below

COMMENT: While the double burden of malnutrition in LMICs is an important topic, the use of this work to inform practice or policy is quite limited.

RESPONSE: The basis of the reviewer's judgement above is unclear. Nevertheless, we assumed this is in relation to their comments below, and which we have addressed accordingly.

The major concerns are:

COMMENT: Part of this topic has been covered before:

Neupane S, Prakash KC, Doku DT. Overweight and obesity among women: analysis of demographic and health survey data from 32 Sub-Saharan African Countries. BMC Public Health. 2016 Jan 13;16:30

RESPONSE: The limitation of the above referenced paper is that it focuses only on overnutrition to the neglect of undernutrition, which is major problem in sub-Saharan Africa. We've also noted the reviewer's admission that the above paper did not fully covered the topic. And this is the gap (the essence of research) our paper addresses by analysing under-and –over nutrition simultaneously, using a more comprehensive list of predictor variables.

COMMENT: Why did the authors only select five SSA countries? There is no explanation how countries are selected.

RESPONSE: To address the first bit of the comment, the decision on the number of datasets or countries to analyse is at the discretion of the researcher. Over several years, we have analysed the DHS data using both single and multiple countries datasets and at no time did we justify why we were analyzing single or multiple countries datasets (at least 4 of these papers have been published in the BMJ open). This is also the case in the literature. For the second bit of the comment, the current work is built on our previous analysis (<https://www.cambridge.org/core/journals/public-health-nutrition/article/differential-effects-of-dietary-diversity-and-maternal-characteristics-on-linear-growth-of-children-aged-659-months-in-subsaharan-africa-a-multicountry-analysis/4CB59435121A1F240FFE6613973F6B11>)

COMMENT: The authors use a limited concept of double burden of malnutrition. There is no consideration of stunting and micro-nutrient deficiency.

RESPONSE: This is inaccurate. The following is the WHO definition of the double burden of malnutrition "The double burden of malnutrition is characterised by the coexistence of undernutrition along with overweight and obesity, or diet-related noncommunicable diseases, within individuals, households and populations, and across the lifecourse" (it is either or). Double refers to two things co-existing. You cannot say double burden is the co-existence of underweight, overweight/obesity and micronutrients deficiency. This is what we call triple burden. For the issue of stunting, we'll like to indicate here that we usually compute stunting when running analysis using children or adolescents data. And it is estimate using height-for-age z-scores<-2 SD (chronic malnutrition). The other indicators are weight-for-height z-scores<-2 SD (wasting or thinness) and weight-for-age z-scores<-2 SD (underweight). BMI-for-age can also be used to estimate overweight, thinness etc. Indeed, even if we had isolated adolescents from the data (as there are no growth charts for adults 20yrs and above) and computed their height-for-age z-scores or BMI-for-age, it would not have made any difference as the double burden will still be stunting/thinness/underweight and overweight/obesity. This is because stunted/thin individuals are likely to be underweight and vice versa. Further, this paper focuses on women of reproductive age, defined in the DHS data as 15-49ys olds, and the BMI data are calculated based on this age group. Based on the above, it is not clear to us why the reviewer felt we needed to have included stunting or micronutrients deficiency in order to broaden the definition.

COMMENT: There is no justification for choice of predictors. For e.g. why is frequency of watching tv or marital status associated with thinness or overweight/obesity?

RESPONSE: We did explain in the manuscript what informed the choice of the predictor variables—review of the literature and statistical analysis.

COMMENT: Many tables with little consideration on how to appropriately present the results. For e.g. are both 95% CI and p-values needed?

RESPONSE: This statement is quite general. Not clear which specific table the reviewer is referring to. In our view, all the tables are appropriately presented and we have published several papers (some in BMJ open) using this presentation style—present coefficients and confidence intervals in tables and use stars to denote the level of statistical significance.

Minor comment:

COMMENT: There is unnecessary detail on equations which is unlikely to be appreciated by readers of the journal.

RESPONSE: Per the BMJ open editorial policy, we think this judgement should be left to reader. Nevertheless, we feel that there is the need to let the reader appreciate the mathematics behind the modeling and we are sure there would be some readers who would be very much interested in this.

General responses: This reviewer basically criticised the paper without any scientific substance. Their reviews are therefore quite unhelpful to the authors. The main objective of peer review is to help the authors to improve on their work but not just to criticize (the easiest thing to do) without making scientific suggestion on how the work should be improved. A reviewer may criticize but the criticism should be scientific with the sole aim to improve the work but not unhelpful general statements or criticize for its sake. I have been a peer reviewer for several reputable international peer reviewed journals (including the BMJ open) over the years and my understanding is that Editors invite reviewers to use their expertise to help improve the work of authors and not just to criticize.

Reviewer: 2

Reviewer Name: Tiffany Gill

Institution and Country: The University of Adelaide, Australia

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Thank you for the opportunity to review this paper. My comments are as follows:

Abstract: Appropriate.

COMMENT: : Introduction: Last line, first paragraph, should be "increases the risk of death"

Second paragraph, page 5, line 10 should be "being overweight as a mother.."

line 13, "long-term weight excess.." should be "long-term excessive weight.

Introduction is appropriate and clear.

RESPONSE: The reviewer's suggestions have been incorporated into the manuscript

COMMENT: Methodology: Page 9 line 21, multivariate should be multivariable

The mathematical description of multinomial logistic regression could be included as supplementary information.

RESPONSE: Multivariate changed to multivariable. This has been done throughout the manuscript. Regarding the mathematical description of the model, though the reviewer suggestion is valid, we thought that it would be easy for the reader to relate the modeling and the mathematics behind it if we had it in the text rather than as a supplementary document.

COMMENT: The predictor variables of interest are described on page 9. It is then suggested in the last paragraph of the methods that there were no candidate predictor variables.

Perhaps it should just be stated that all predictors were entered into the model.

RESPONSE: As suggested by the reviewer the sentence now reads "All covariates were simultaneously entered into the model"

COMMENT: Sampling is well described and it is noted that the data are standardized to allow cross country comparison but what are the data standardized to?

RESPONSE: Thank you for this positive comment. The DHS use standard questionnaire across all countries, and thus the data collected are the same for all countries except in few cases where there may be added country specific variables. Also, the variable names are exactly the same across countries. This is what we meant by DHS data are standardized.

COMMENT: There is no statement regarding ethics or consent for these studies and this should be added.

RESPONSE: This has now been included in the manuscript "The DHS obtained ethical clearance from the ethical committees of the respective countries before the surveys were conducted. Written informed consent was obtained from the women before participation. The authors of this paper sought and obtained permission from the DHS program for the use of the data. The data were completely anonymized and therefore the authors did not seek further ethical clearance before their use"

COMMENT: Results: Descriptive section, line 21, should be "Women in Kenya."

Line 24 "fairly the same" should be "similar"

RESPONSE: The correction has been effected as suggested by the reviewer

COMMENT: Multivariate section, page 13, should be Multivariable. This should also be changed throughout.

RESPONSE: This has been done throughout the manuscript

COMMENT: Tables are appropriate.

RESPONSE: Thank you for this positive comment

COMMENT: Discussion: Has SSA been defined in the text?

RESPONSE: SSA has now been defined

COMMENT: Page 22 line 3, should "dietary patterns" be "poor dietary patterns"

Line 6 "Contrariwise" should be "In contrast"

RESPONSE: Changes have been effected as suggested by the reviewer

COMMENT: Line 15 "associate" should be "associated"

Some other issues with English impact on the clarity with the discussion.

RESPONSE: We have gone through the discussion section to ensure that any detected grammatical error is addressed

COMMENT: Conclusion: Appropriate, however stating "old age" is significantly associated is somewhat problematic. The age range (15-49) in this study may be "old age" however may not be "old age" for the reader. It would be useful to put the age range used in this study into context of life expectancy for these countries. This perhaps could be done in the background.

RESPONSE: Thank you for this comment. We agree with the reviewer that the use of the phrase "old age" is a bit confusing considering the age group of the study participants. We did not mean old people or the aged, what we meant is the effect of changes in a unit of age or age difference within the 15-49 age group e.g. moving from right to left—30 is older than 29, 45 older than 44 etc. To address the concern of the reviewer, we have revised the sentence to read, "a unit change in age is associated with....."

Reviewer: 3

Reviewer Name: Carla Alberici Pastore

Institution and Country: Federal University of Pelotas - Brazil

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

COMMENT: Congrats to the authors, it is a very interesting and well writing manuscript.

I have only a few suggestions to point, in the better intention of improve the manuscript:

RESPONSE: Thank you for this positive comment

COMMENT: The WHO BMI classification used in the study was designed for adult people (18 years old and over). But the sample contains women between 15 and 18 years old. For those, WHO suggests to use the Growth Standards Curves, which allows to adolescents to present lower BMI without being underweight? So, the use of adult's BMI cut points could had overestimated the prevalence of undernutrition in this subsample of under 18 years' old women.

RESPONSE: We appreciate the concern of the reviewer. However, we did not compute BMI-for-age which may make the adults and adolescents to have different cut-off points. As the reviewer may be aware, the BMI-for-age z-scores is what the WHO used to estimate the growth charts for adolescents (height-for-age; weight-for-age and weight-for-height has also been used). Since we used only height and weight to estimate the BMI, the cut-off points are the same for both adolescent girls and women. Secondly, the DHS has grouped all females' aged 15-49 years as women of reproductive age and hence lumped the BMIs of this group together and call it BMI for women of reproductive age. All the papers we have reviewed or published ourselves on BMI using the DHS data used the same cut-off point for this age group.

COMMENT: On table 2, I suggest to include the Standard Deviation (DP), and not the Standard Error, along with the mean values (in continuous variables).

RESPONSE: This has now been done. Revised table included in the manuscript

COMMENT: Still on table 2, the term "Currently breastfeeding" in the first column should be in bolded font.

RESPONSE: It is not a sub-heading and does not have to be bold



## VERSION 2 – REVIEW

<b>REVIEWER</b>	Tiffany Gill The University of Adelaide, Australia
<b>REVIEW RETURNED</b>	28-Apr-2019

<b>GENERAL COMMENTS</b>	<p>I thank the authors for addressing the previous comments. My further comments are as follows.</p> <p>Abstract: Appropriate.</p> <p>Introduction: Clear.</p> <p>Methodology: Page 7, line 38: "...5 years and standardized to..." It should be stated in the manuscript "5 years using standardized questionnaires to enable..." This eliminates the potential confusion of standardizing the results to a population to enable comparisons. Page 8, line 31: "ethical committees of the respective countries". Could the authors please just clarify further. For example, were these Government Health Department committees or University based. Generally, a country does not have their own ethics committee.</p> <p>Page 9, line 8: Height does not need a capital.</p> <p>Page 10: While I understand the authors desire to include their analytical strategy and formula within the document, I remain unconvinced that it assists the ordinary reader and if readers are truly interested they will go to the supplementary information.</p> <p>Results: Appropriately described.</p> <p>Discussion: Appropriate.</p> <p>Tables and figures clear.</p>
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<b>REVIEWER</b>	Carla Alberici Pastore Federal University of Pelotas - Brazil
<b>REVIEW RETURNED</b>	25-Apr-2019

<b>GENERAL COMMENTS</b>	In my opinion, the mathematics equations are not necessary in the Statistics presentation. Just to name the tests used is enough.
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## VERSION 2 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 3

Reviewer Name: Carla Alberici Pastore

Institution and Country: Federal University of Pelotas - Brazil

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

COMMENT: In my opinion, the mathematics equations are not necessary in the Statistics presentation. Just to name the tests used is enough.

RESPONSE: Per the suggestion of the Editor, this has been maintained

Reviewer: 2

Reviewer Name: Tiffany Gill

Institution and Country: The University of Adelaide, Australia

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

I thank the authors for addressing the previous comments. My further comments are as follows.

COMMENT: Abstract: Appropriate.

RESPONSE: Thank you for this positive comment

COMMENT: Introduction: Clear.

RESPONSE: Thank you for this positive comment

COMMENT: Methodology: Page 7, line 38: "...5 years and standardized to..." It should be stated in the manuscript "5 years using standardized questionnaires to enable..." This eliminates the potential confusion of standardizing the results to a population to enable comparisons.

RESPONSE: This has been added to the manuscript as suggested by the reviewer. "...5 years using standardized questionnaires to enable cross-country comparisons"

COMMENT: Page 8, line 31: "ethical committees of the respective countries". Could the authors please just clarify further. For example, were these Government Health Department committees or University based. Generally, a country does not have their own ethics committee.

RESPONSE: The sentence has been revised and now reads "The DHS obtained ethical clearance from Government recognised Ethical Review Committees/Institutional Review Boards of the respective countries as well as the Institutional Review Board of ICF International, USA, before the surveys were conducted"

COMMENT: Page 9, line 8: Height does not need a capital.

RESPONSE: This has been corrected

COMMENT: Page 10: While I understand the authors desire to include their analytical strategy and formula within the document, I remain unconvinced that it assists the ordinary reader and if readers are truly interested they will go to the supplementary information.

RESPONSE: Per the suggestion of the Editor, this has been maintained

COMMENT: Results: Appropriately described.

RESPONSE: Thank you for this positive comment

COMMENT: Discussion: Appropriate.

RESPONSE: Thank you for this positive comment

COMMENT: Tables and figures clear.

RESPONSE: Thank you for this positive comment