PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Epidemiology of Obesity and Overweight in Sub-Saharan Africa: A Protocol for a Systematic Review and Meta-Analysis
AUTHORS	Biadgilign, Sibhatu; Mgutshini, Tennyson; Haile, Demewoz; Gebremichael, Bereket; Mesfin, Yonatan; Kibret, Kelemu

VERSION 1 – REVIEW

REVIEWER	John W Stanifer Duke University, United States
REVIEW RETURNED	12-May-2017

GENERAL COMMENTS	The authors present a protocol for a proposed study examining the prevalence of obesity in Sub-Saharan Africa. This is an important question and the design is appropriate to answer the question; however, I have a few comments/suggestion which may enhance the impact and usefulness of the study. 1) The authors propose in the introduction that childhood obesity may deserve extra merit and attention. This indeed is a growing problem and worth examining, and their proposed search strategy makes a distinction between adults and children. However, the analysis portion does not mention reporting childhood vs. adult prevalence of overweight/obesity. In my opinion it would be very much worth stratifying the analysis to look at differences. 2) Similar to my first comment, the authors state that urbanization and demographic transitions are an important factors in the growing obesity epidemic in sub-Saharan Africa (and I agree). The analysis, however, should be built more around answering this question; as such, it may be worth stratifying the analysis by urban vs. rural studies to report on differences in obesity epidemiology by level or urbanicity. This could be very valuable and would add greatly to our understanding of NCDs in sub-Saharan Africa. 3) Will the statistical analysis report just an overall prevalence? Or will it also report prevalence by country? Also should consider reporting prevalence by rural vs. urban and child vs. adult as suggested above. Also how will overweight vs. obesity be analyzed? Will there be a prevalence estimate for each one separately? And how will they be defined? 4) Regarding the Search strategy: a. Would review the search terms for sub-Saharan Africa. We used the following in our systematic review, so may be worth comparing: ("Africa South of the Sahara"[Mesh] OR "ivory coast"[tiab] OR Cameroon[tiab] OR Central African Republic[tiab] OR Chad[tiab] OR Congo[tiab] OR Democratic Republic of the Congo[tiab] OR Equatorial

	Guinea[tiab] OR Gabon[tiab] OR Burundi[tiab] OR Djibouti[tiab] OR Eritrea[tiab] OR Ethiopia[tiab] OR Kenya[tiab] OR Rwanda[tiab] OR Somalia[tiab] OR Sudan[tiab] OR Tanzania[tiab] OR Uganda[tiab] OR Angola[tiab] OR Botswana[tiab] OR Lesotho[tiab] OR Malawi[tiab] OR Mozambique[tiab] OR Namibia[tiab] OR South Africa[tiab] OR Swaziland[tiab] OR Zambia[tiab] OR Zimbabwe[tiab] OR Benin[tiab] OR Burkina Faso[tiab] OR Cape Verde[tiab] OR Cote d'Ivoire[tiab] OR Gambia[tiab] OR Ghana[tiab] OR Guinea[tiab] OR Guinea-Bissau[tiab] OR Liberia[tiab] OR Mali[tiab] OR Mauritania[tiab] OR Niger[tiab] OR Nigeria[tiab] OR Senegal[tiab] OR Sierra Leone[tiab] OR Togo[tiab] OR (subsaharan[tiab] AND africa[tiab]) b. The search terms for obesity/overweight are not comprehensive enough. Would include body mass index, body fat, adiposity, and other commonly used terms for measuring weight. Several studies may only report a BMI and just using obesity/overweight may miss quite a few. c. In the text (page 5, line 25), the authors state that 'prevalence' and 'epidemiology' are search terms, but they are not in the Appendices. I do not think they need to be included anyway but should be removed in that case from the text. d. Is there anyway to not filter on English language? Several studies may be reported in French or other language, which could miss quite a few. Even if the authors do not have the ability to translate all studies, they should do a search with and without the language filter to understand how many they miss by filtering on language. 5) The authors should provide more detail about inclusion/exclusion criteria. One of the biggest challenges will be that most studies are performed in the hospital or clinics, rather than purely community-based epidemiological studies. Are all of these studies to be included? And if so, how can a hospital-based study help us understand epidemiology of obesity? Would consider making that an exclusion criterion. And what about case-control studies? Will authors include those so that they can examine one of the arms for prevalence? And what about studies that report an average BMI or have data available for estimating obesity, but they do not report an overall estimate? Will the authors themselves do those calculations? For example, in CKD-ARIKA Study, we have reported population-level BMI estimates in several manuscripts, but we have not reported an overall obesity prevalence, though this could be done from what we have reported. The authors should consider how to handle such situations. 6) Would consider adding a flow diagram to the Methods Section to show how each step will be done? The numbers won't be available yet, but it may make things more clear. Also, the authors should assess inter-rater agreement for inclusion/exclusion at each step. 7) The statistical analyses for heterogeneity should be done and placed before description of any meta-analyses. My biggest concern will be the degree of heterogeneity that is observed, and how will the authors handle this. What will be an acceptable I2 for performing the meta-analyses? The authors should state this a priori. And how will quality be incorporated into the analyses? Will only high-quality studies be meta-analyzed? Or will they be weighted differently, and if so, how? What about studies performed in much different time-periods? The searches are designed to go back as far as 1950, so will studies from the 1950s be included equally as with studies from 2000s?

	8) Another important question for meta-analysis will be how to handle differing definitions of overweight/obesity. For example, what is one study uses BMI>25, another BMI>30 and one uses a completely different assessment such as % body fat? How will the authors handle this? 9) For quality assessment, I would review [The epidemiology of chronic kidney disease in sub-Saharan Africa: a systematic review and meta-analysis] and consider incorporating those quality criteria, and as I mentioned above, consider what the impact of quality will be on the analyses. 10) I am not sure what the need for meta-regression is. What are the authors trying to model and what questions are they trying to answer? Are they trying to understand risk factors? I say this because meta-regression will be very challenging given the heterogeneity and presumably high missingness of data. 11) Grammar/writing: the authors should review the written text carefully. Several spelling errors are noted (e.g. page 4, line 49 'death' instead of 'dearth') and several sentences are run-on and difficult to read (e.g. page 5, lines 13-22). Overall, the study will add to the literature and answer important questions. I have added these comments here to try to enhance the impact and quality of the authors' work.

REVIEWER	Jenny Cresswell London School of Hygiene & Tropical Medicine, UK
REVIEW RETURNED	19-May-2017

GENERAL COMMENTS	The protocol should explicitly define the outcome (overweight and obesity) in terms of kg/m2 in the protocol. This is currently missing. Also make clear how they will be defining obesity in children (on weight-for-height, weight-for-age, or otherwise?) I suggest that the authors add a comprehensive search/review of the DHS Programme website to their search strategy (www.measuredhs.com). The standard questionnaire for the DHS has included height and weight of household members for a number of years – so BMI/overweight/obesity is usually included in a table in the Final Report. DHS is one of the most complete and nationally-representative sources of data on body size in Africa. DHS Final Reports are not typically indexed in PubMed etc so these may be missed without a specific website search. The authors should consider publications in French or they will bias their findings by not identifying relevant sources from West Africa. Also Portuguese for Mozambique, Angola etc. There are grammatical errors through the manuscript which should be checked.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
The authors present a protocol for a proposed study examining the prevalence of obesity in Sub-Saharan Africa. This is an important question and the design is appropriate to answer the question; however, I have a few comments/suggestion which may enhance the impact and usefulness of the study.

Comment 1: The authors propose in the introduction that childhood obesity may deserve extra merit and attention. This indeed is a growing problem and worth examining, and their proposed search strategy makes a distinction between adults and children. However, the analysis portion does not mention reporting childhood vs. adult prevalence of overweight/obesity. In my opinion it would be very much worth stratifying the analysis to look at differences.

Response: The proposed revisions made by the reviewer have been adopted and results have been stratified and separated out by demographic group.

Comment 2: Similar to my first comment, the author's state that urbanization and demographic transitions are important factors in the growing obesity epidemic in sub-Saharan Africa (and I agree). The analysis, however, should be built more around answering this question; as such, it may be worth stratifying the analysis by urban vs. rural studies to report on differences in obesity epidemiology by level or urbanicity. This could be very valuable and would add greatly to our understanding of NCDs in sub-Saharan Africa.

Response. The findings have been revised and now separate out discoveries between urban and rural populations, as requested by the reviewer.

Comment 3: Will the statistical analysis report just on overall prevalence or will it also report prevalence by country? Also should consider reporting prevalence by rural vs. urban and child vs. adult as suggested above. Also how will overweight vs. obesity be analyzed? Will there be a prevalence estimate for each one separately? And how will they be defined?

Response: The suggested actions have been taken full count of and revisions have been made in line with the comments by the reviewer. In doing this, we will estimate pooled prevalence rates of obesity and overweight for different groups such as urban vs rural, children vs adult etc, We will explicitly perform sub group analyses and where possible, analyses per region (western, eastern southern).

Comment 4: Regarding the Search strategy:
a. Would review the search terms for sub-Saharan Africa. We used the following in our systematic review, so may be worth comparing:
("Africa South of the Sahara"[Mesh] OR "ivory coast"[tiab] OR Cameroon [tiab] OR Central African Republic[tiab] OR Chad[tiab] OR Congo[tiab] OR Democratic Republic of the Congo[tiab] OR Equatorial
Guinea[tiab] OR Gabon[tiab] OR Burundi[tiab] OR Djibouti[tiab] OR Eritrea[tiab] OR Ethiopia[tiab] OR Kenya[tiab] OR Rwanda[tiab] OR Somalia[tiab] OR Sudan[tiab] OR Tanzania[tiab] OR Uganda[tiab] OR
Angola[tiab] OR Botswana[tiab] OR Lesotho[tiab] OR Malawi[tiab] OR Mozambique[tiab] OR Namibia[tiab] OR South Africa[tiab] OR Swaziland[tiab] OR Zambia[tiab] OR Zimbabwe[tiab] OR Benin[tiab] OR Burkina Faso[tiab] OR Cape Verde[tiab] OR Cote d'Ivoire[tiab] OR Gambia[tiab] OR Ghana[tiab] OR Guinea[tiab] OR Guinea-Bissau[tiab] OR Liberia[tiab] OR Mali[tiab] OR Mauritania[tiab]
OR Niger[tiab] OR Nigeria[tiab] OR Senegal[tiab] OR Sierra Leone[tiab] OR Togo[tiab] OR (subsaharan[
tiab] AND Africa[tiab])

Response: Many thanks indeed for your proposed search strategy. We will align this one with the one proposed and check the final result.

Comment b. The search terms for obesity/overweight are not comprehensive enough. Would include body mass index, body fat, adiposity, and other commonly used terms for measuring weight. Several studies may only report a BMI and just using obesity/overweight may miss quite a few.

Response: This comment is acknowledged and has been explicitly addressed in the revised manuscript.

Comment c. In the text (page 5, line 25), the authors state that 'prevalence' and 'epidemiology' are search terms, but they are not in the Appendices. I do not think they need to be included anyway but should be removed in that case from the text.

Response: The reviewer's comment has been accepted and content is removed in the current version.

Comment d. Is there any way to not filter on English language? Several studies may be reported in French or other language, which could miss quite a few. Even if the authors do not have the ability to translate all studies, they should do a search with and without the language filter to understand how many they miss by filtering on language.

Response: Actually our focus is on English language due to the fact that the issue might rise on translation of the articles and documents which is beyond the author's knowledge. However, as per the reviewer suggestions, we will include an acknowledgement of non-English sources in our search approach and on current manuscript.

Comment 5: The authors should provide more detail about inclusion/exclusion criteria. One of the biggest challenges will be that most studies are performed in the hospital or clinics, rather than purely community-based epidemiological studies. Are all of these studies to be included? And if so, how can a hospital-based study help us understand epidemiology of obesity? Would consider making that an exclusion criterion. And what about case-control studies? Will authors include those so that they can examine one of the arms for prevalence? And what about studies that report an average BMI or have data available for estimating obesity, but they do not report an overall estimate? Will the authors themselves do those calculations? For example, in CKD-ARIKA Study, we have reported population-level BMI estimates in several manuscripts, but we have not reported overall obesity prevalence, though this could be done from what we have reported. The authors should consider how to handle such situations.

Response: The reviewer's suggestions are noted and integrated into the revised manuscript. In order to capture a large volume of studies, we are tried to include any epidemiological studies with their characters. For the case of the case control, we will focus on the prevalence data from one of the arms reported in their study. We were requested for those which do not have an overall prevalence estimate; we will include calculating by ourselves and also contacting the corresponding authors to forward the analyzed data. We will also report the overall and country specific prevalence data from the estimate and calculated one.

Concerning the hospital or clinics studies; offshore such studies could not produce quality epidemiological date compared to community-based study but these would provide a clue to have good understanding on epidemiology of obesity and overweight. So we will include all hospital and community based studies and we can carry out subgroup analysis.

Comment 6: Would consider adding a flow diagram to the Methods Section to show how each step will be done? The numbers won't be available yet, but it may make things more clear. Also, the authors should assess inter-rater agreement for inclusion/exclusion at each step.

Response: It is a good suggestion. However, we are planning to include flow chart diagram after finalizing the search strategy from the mentioned database.

Comment 7: The statistical analyses for heterogeneity should be done and placed before description of any meta-analyses. My biggest concern will be the degree of heterogeneity that is observed, and how will the authors handle this. What will be an acceptable I2 for performing the meta-analyses? The authors should state this a priori. And how will quality be incorporated into the analyses? Will only high-quality studies be meta-analyzed? Or will they be weighted differently, and if so, how? What about studies performed in much different time-periods? The searches are designed to go back as far as 1950, so will studies from the 1950s be included equally as with studies from 2000s?

Response: It is a good observation. In fact, heterogeneity is a common phenomenon when we conduct any meta-analysis as studies differ by virtue of differing settings and parameters of measurements. It is clearly stated in the method section under Statistical heterogeneity part. We will use different mechanisms to handle the heterogeneity. The recommended approaches to handle the heterogeneity problems are using the random effect methods. It is ok we analyses using random effect models which takes in to consideration the variability between studies. The other mechanism that we will apply to handle this issue is investigating the source of heterogeneity by sub group analysis. All papers will be evaluated for quality to be included and analyzed accordingly. We did not ignore any articles even if it is low low-quality studies. We can conduct sensitivity analysis including and excluding low quality studies.

Comment 8: Another important question for meta-analysis will be how to handle differing definitions of overweight/obesity. For example, what is one study uses BMI>25, another BMI>30 and one uses a completely different assessment such as % body fat? How will the authors handle this?

Response: It is a good comment. For the case of clarification, we will analyze separately for overweight and obesity. Along with this, we also consider the criteria for classification of overweight and obesity based on the available evidence that the studies used to classify for overweight and obesity accordingly.

Comment 9: For quality assessment, I would review [The epidemiology of chronic kidney disease in sub-Saharan Africa: a systematic review and meta-analysis] and consider incorporating those quality criteria, and as I mentioned above, consider what the impact of quality will be on the analyses.

Response: it is a good suggestion and we will accommodate to include the aforementioned quality assessment in the revised manuscript.

Comment 10: I am not sure what the need for meta-regression is. What are the authors trying to model and what questions are they trying to answer? Are they trying to understand risk factors? I say this because meta-regression will be very challenging given the heterogeneity and presumably high missingness of data.
Response: Nice comment and observations regarding the meta regression. The issue of missing data and other unforeseen factors might increase the challenges related to doing this. However, we are proposing to investigate some of the factors that contribute to for overweight and obesity.

Comment 11: Grammar/writing: the authors should review the written text carefully. Several spelling errors are noted (e.g. page 4, line 49 ‘death’ instead of ‘dearth’) and several sentences are run-on and difficult to read (e.g. page 5, lines 13-22). Overall, the study will add to the literature and answer important questions. I have added these comments here to try to enhance the impact and quality of the authors’ work.

Response: At this time, several revisions were undertaken and edited by different people and many thanks for the reviewer thought.

Reviewer: 2

Comment 1: The protocol should explicitly define the outcome (overweight and obesity) in terms of kg/m2 in the protocol. This is currently missing. Also make clear how they will be defining obesity in children (on weight-for-height, weight-for-age, or otherwise?)

Response: It is a good comment. Actually, we will try to check the use different cut off marks for both obesity and overweight in terms of BMI. Additionally, we will take in to consideration the different cut off points suggested by CDC, International Obesity Task Force, and WHO. There is no clear gold standard measurement that shows for identity overweight and obesity in the certain population segments. Here again, we will capture what is available in the paper that distinguish overweight and obesity according to their preference and way of applicability for categorization by authors. I.e. CDC, International Obesity Task Force, and WHO.

Comment 2: I suggest that the authors add a comprehensive search/review of the DHS Programme website to their search strategy (www.measuredhs.com). The standard questionnaire for the DHS has included height and weight of household members for a number of years – so BMI/overweight/obesity is usually included in a table in the Final Report. DHS is one of the most complete and nationally-representative sources of data on body size in Africa. DHS Final Reports are not typically indexed in PubMed etc so these may be missed without a specific website search.

Response: it is a good suggestion and included in the revised manuscript.

Comment 3: The authors should consider publications in French or they will bias their findings by not identifying relevant sources from West Africa. Also Portuguese for Mozambique, Angola etc.

Response: The same comment was asked by the first reviewer. And responded in the above list under question in 4.d.

Comment 4: There are grammatical errors through the manuscript which should be checked.

Response: the issue raised is addressed in the revised manuscript.

VERSION 2 – REVIEW

REVIEWER	John W Stanifer Duke University No competing interest
REVIEW RETURNED	02-Aug-2017

GENERAL COMMENTS	Overall, the authors seek to answer an important question and this should make for a meaningful impact article once conducted. I have a few minor comments still yet. 1) The authors have not updated Appendix 1 to include the additional terms for obesity/overweight. 2) The authors should state more clear what they will do if (or when) they find substantial heterogeneity. Is there a level at which they will not do meta-analysis? or would they stratify or restrict to certain studies?) 3) the quality assessment will be very important, but how will the authors use it? will they only use high (or high and medium) studies in the meta-analysis? This would be a reasonable approach. 4) in inclusion/exclusion, the authors should state whether they are only including community-based studies? and will they include RCTs or observational cohort studies where baseline prevalence estimates are reported? 5) I am still not sure whey a meta-regression is being considered? the authors will explore confounders but of what relationship? what are the dependent and independent variables to be studied by these methods?

REVIEWER	Jenny Cresswell London School of Hygiene & Tropical Medicine, UK
REVIEW RETURNED	14-Aug-2017

GENERAL COMMENTS	Thank you for addressing my comments - good luck with the review.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: John W Stanifer
Institution and Country: Duke University

Please state any competing interests or state ‘None declared’: no competing interest

Please leave your comments for the authors below
Overall, the authors seek to answer an important question and this should make for a meaningful impact article once conducted.
I have a few minor comments still yet.
1) The authors have not updated Appendix 1 to include the additional terms for obesity/overweight.

Response: It is a good comment and we have added new terms for searching in the selected database.

2) The authors should state more clear what they will do if (or when) they find substantial heterogeneity. Is there a level at which they will not do meta-analysis? or would they stratify or restrict to certain studies?)

Response: we have already explained in the manuscript on handling heterogeneity and added that the low quality studies will be removed from analysis.

3) The quality assessment will be very important, but how will the authors use it? Will they only use high (or high and medium) studies in the meta-analysis? This would be a reasonable approach.

Response: we have already addressed the raised point in the previous version. In addition to that, we have removed low quality studies from analysis.

4) In inclusion/exclusion, the authors should state whether they are only including community-based studies? And will they include RCTs or observational cohort studies where baseline prevalence estimates are reported?

Response: it is a well taken feedback and actually the information about the inclusions and exclusions criteria is well displayed in the body of the manuscript. The papers will also been included both community and institutional based studies. For the sake of clarity, we posted again here.
Studies will be required to meet the following inclusion criteria: Reports of original studies, unpublished master's thesis and PhD dissertations written in English, French or other languages and findings evaluated for prevalence of obesity and overweight and studies coming from sub-Sahara African settings will also be considered. All epidemiological studies of observational (cross-sectional, cohort, case-control) and randomized controlled trial will be included. Studies will be excluded from the analysis for any of the following reasons: duplicate publication of the same study; and articles available only in abstract form, letters, reviews, commentaries, editorials, case series with small sample size (<30 participants), studies with no appropriate BMI or BMI z score determination and those studies conducted outside of sub-Saharan Africa will be excluded.

5) I am still not sure why a meta-regression is being considered? The authors will explore confounders but of what relationship? What are the dependent and independent variables to be studied by these methods?

Response: it is a good comment and we have removed the statement about meta-regression idea from the manuscript.

Reviewer: 2
Reviewer Name: Jenny Cresswell
Institution and Country: London School of Hygiene & Tropical Medicine, UK
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Comment: Thank you for addressing my comments - good luck with the review.

Response: Many thanks for your understanding.

VERSION 3 – REVIEW

REVIEWER	John W Stanifer Duke University, United States No competing interests
REVIEW RETURNED	18-Sep-2017

GENERAL COMMENTS	overall, the authors seek to answer an important question and the Methods are appropriate. I have just a few minor comments, 1) the authors state the published and unpublished studies will be included, what strategy do they have for identifying unpublished studies? 2) authors state studies in 'other languages' will be included. Should be more specific here. 3) the dates in the Study design for abstraction should be updated 4) for purposes of their statistical analysis, authors should state how they will define obesity and overweight. 5)is there a level of I2 which the authors will consider unacceptable to perform a meta-analysis?

VERSION 3 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: John W Stanifer
Institution and Country: Duke University, United States
Please state any competing interests or state ‘None declared’: No competing interests

Overall, the authors seek to answer an important question and the Methods are appropriate. I have just a few minor comments,

Comment 1) the authors state the published and unpublished studies will be included, what strategy do they have for identifying unpublished studies?

Response: it is a good comment and we have revised to remove the unpublished studies from the body of MS.

Comment 2) Authors state studies in 'other languages' will be included. Should be more specific here.

Response: Actually we want try to include other languages if there is a possibility to increase and making without any restriction of our search. But we think it is better to include English and French as widely spoken in Africa countries.

Comment 3) The dates in the Study design for abstraction should be updated

Response: It is a good comment and we revised from 1947 to September 30, 2017.

Comment 4) For purposes of their statistical analysis, authors should state how they will define obesity and overweight.

Response: It is good feedback suggested. Apparently we revised in the revised version of the manuscript as" Obesity and overweight will be defined in this study based on body mass index cut-off value from any of the established standard definition for child overweight and obesity measurement: Centers for Disease Control and Prevention(CDC), International Obesity Task Force (IOTF) and The World Health Organization (WHO).

Comment 5) Is there a level of I2 which the authors will consider unacceptable to perform a meta-analysis?

Response: it is a good comment and it is arbitrary with a common sense to handle as there is no realistic cut of value/level stated in literatures. However, we still use the traditional/ convention form of acceptability with the following context: I2 value of less than 25% as good, 25% to 50% is acceptable and more than 50% as unacceptable.

VERSION 4 – REVIEW

REVIEWER	John Stanifer Duke University, United States
REVIEW RETURNED	29-Sep-2017

GENERAL COMMENTS	The authors have addressed all concerns.