Author’s response to reviews

Title: Prevalence and Determinants of Gestational Diabetes Mellitus in Africa Based on the Updated International Diagnostic Criteria: A systematic review and meta-analysis

Authors:

Achenef Muche (ashua2014@gmail.com)

Oladapo Olayemi (oladapo.olayemi@yahoo.com)

Yigzaw Gete (gkyigzaw@yahoo.com)

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Author’s response to reviews:

Point by point response to reviewers

Dear reviewers,

We duly acknowledge your efforts to give us very interesting and constructive comments on our manuscript. We have meticulously read all your comments and we have addressed almost all comments that you gave for us. The major revisions we made and the reflections we gave for your questions and comments are stated as follows;

Reviewer #1 Comments

Abstract

Comment 1: Page 2, line 17: "was" instead of "were"

Response: The comment has been accepted and corrected. (see page 1, line 6)

Comment 2: Line 21: information about subgroup should not appear in the background section

Response: The comment has been accepted and corrected. The sentence has been removed.

Comment 3: Line 25: "is" instead of "used
Response: The comment has been accepted and corrected.

Comment 4: Line 44: define abbreviations, even GDM
Response: The comment has been accepted and corrected. The abbreviations has defined at background section of the abstract (see page 1, line 2).

Comment 5: Line 49: no results of heterogeneity is given in the abstract section
Response: The comment has been accepted and corrected. The $I^2$ has has been added. (see page 1, line 16-17).

Introduction

Comment 1: a previous SR and MA has already been performed. What is the difference between this previous work and the current work exactly? Please provide information about heterogeneity of this previous MA.

Response: We found out that your question is very interesting. Previous review included studies only in sub-Saharan African countries with lack of uniformity in screening methods, definition, and diagnostic criteria for GDM makes it difficult to compare the prevalence of GDM between and within countries and point out the true pooled prevalence. Furthermore, the review did not included studies conducting by using updated or current diagnostic criteria (WHO 2013), and did not report findings on risk factors for GDM based in the new diagnostic criteria (WHO 2013) and again the review without meta-analysis (only narratives) included few studies did not investigate the sources of heterogeneity between the studies, uses different diagnostic criteria to define GDM and did not report findings on risk factors for GDM. (see page 4, line 11-19)

Methods

Comment 1: Did the authors write a protocol prior doing this SR research? If yes, please provide reference for this protocol.

Response: Yes, the review was registered with PROSPERO (2018:CRD42018116843).A protocol was developed during the planning process. (see page 5, line 5-8)
Comment 2: PubMed search: why authors did not used Mesh terms in their search strategy?

Response: A combination of expanded MeSH term and free-text searches were used. (see page 5, line 15-23 and the details have shown in Appendix 1)

Comment 3: Why the authors did collected studies published only from January 2013?

Response: We found out that your question is very interesting. In 2013, WHO revised its recommendations for diagnosis of GDM taking into cognizance the issues raised by the International Association of Diabetes in Pregnancy Study Groups (IADPSG) recommendations. The WHO 2013 modifications along with common diagnostic criteria for GDM by 2013 American Diabetes Association (ADA). Though, in the last five years the diagnostic criteria have been changed and there is no known overall prevalence and associated factors of GDM after the change in Africa. Moreover, as clearly we justified the prevalence of GDM is not only affected by the population characteristic but also by screening and diagnostic criteria. We are interested to identify the pooled prevalence by updated or current diagnostic criteria which is recommended by WHO 2013 and IDASPG and ADA. African countries practices this criteria at this time. Therefore we are confident enough we point out the true and current prevalence of GDM by using similar definition of GDM. (See the title “Based on the Updated International Diagnostic Criteria”, Abstract section (page 1, line 3-5), Background section (page 4, line 5-24) and Method section (page 6, line 4-5 and line 19-23))

Comment 4: Authors should explain more deeply the process of study selection in the appropriate section (and not in the extraction selection).

Response: The comment has been accepted and corrected. We have also added study selection in the appropriate in the method part. (see page 6, line 24-30)

Comment 5: The inclusion criteria are not complete. Indeed, the reviewer suppose that studies had to provide prevalence data as well as data regarding risk factors for GDM.

Response: The comment has been accepted and corrected. We have added the sentence and clearly stated the inclusion criteria. (see page 5, line 25-30 and page 6, line 1-5)

Comment 6: Please provide a reason for excluding studies not published in English language.
Response: As we all know, one of the hard part of systematic review and analysis included and to manage published articles without language restrictions. We understood language bias typically refers to a systematic bias due to the selection of research findings in a particular language. But, due to the language capabilities of the authors (all authors are only English language natives) and limited resources for translation, we have searched only published articles in English language journals as in a way that explained above. We have also mentioned it as limitation of study in the discussion section of the manuscript.

Comment 7: Did the authors pre-tested their Excel extraction sheet?

Response: Yes, We have checked (pretested) the consistency of Excel extraction sheet to have used for summarizing the necessary data extracted from each selected articles. After the quality appraisal and the information needed for the review was collected using the data extraction tool for prevalence studies prepared by the JB. We have used the MS Excel extraction sheet to summarize the findings, calculating the outcome (effect size) each selected articles, calculating standard error (SE) then the aggregated data on Microsoft Excel was exported into STATA/SE version 14 software for Meta-analysis.

Comment 8: The whole paragraph regarding heterogeneity and publication bias should be moved to the statistical method and analysis paragraph.

Response: The comment has been accepted and we have moved the heterogeneity and publication bias section to the statistical method and analysis part. (see page 7, line 21-27)

Comment 9: In the abstract, authors defined heterogeneity when $P>50\%$. The definition of heterogeneity in the methods is not the same. Please be concordant

Response: The comment has been accepted and corrected. We have defined the presence of heterogeneity was considered to I2 test statistics results $>50\%$. We make similar in the abstract and method section. (see abstract section (page 1, line 12) and method section (page 7, line 23))

Results

Comment 1: Authors reported a search in gray literature that has not been presented in the methods section.
Response: The comment has been accepted and corrected. The gray literature we found also have published articles extracts from entire document. We have edited. (see page 8, line 7)

Comment 2: How is it possible to include studies conducted in 2012 when your search limited studies to those published in 2013?

Response: The comment has been accepted and corrected. We have revised sub group analysis by year of publication. Even though, we selected published articles starts 2013 using the updated diagnostic criteria for GDM (as WHO formally accepted the new criteria by 2013). We found studies conducted or year of survey) before and published after 2013. Therefore, initially we have considered the sub group analysis of the study by the year of survey. Now we have revised the sub group analysis by publication year of studies to more appealing the readers about the exact variation of prevalence by the publication year of the study. (see page 9, line 13-15 and Table 2)

Comment 3: The reviewer strongly encourages the authors to original funnel plots as well as funnel plots improved by the trim and fill method in the supplementary files.

Response: The comment has been accepted and We have attached the supplementary files original funnel plots as well as funnel plots improved by the trim and fill method (see the attached supplementary file)

Comment 4: Page 6, line 31, please provide all the CI for the reported prevalence

Response: We thought reviewer question was on Page 9, line 31 rather than Page 6, line 31. The comment has been accepted and we have added CI for the reported prevalence (see page 9, line 13-19)

Comment 5: Page 6, line 39, all this paragraph should be deleted as it is redundant with the next paragraph.

Response: Again, We thought reviewer question was on Page 9, line 39 rather than Page 6, line 39. We agreed on your comment without any reservation and, therefore, this paragraph. Has been deleted.
Comment 6: Did the authors use the same criteria for defining maternal obesity across all studies?

Response: Different studies used different criteria to define maternal overweight and/or obesity. The discrepancy happened across studies due to the nature of pre gestational weight is unknown by most pregnant women. If pre gestational weight is known is criteria by body mass index is accepted, but some studies used mid upper arm circumference (MUAC) to indicate maternal overweight and/or obesity in case of pre gestational weight is unknown. Example articles published by Ogoudjobi et. al, 2017 and Oppong et.al, 2015 used BMI ≥ 25 Kg/m2, while Mwanri et.al, 2014 and Njet et.al, 2018 used MUAC of ≥28 cm was taken to indicate overweight and/or obesity. Evidences revealed that MUAC was recognized as quite stable during the course of pregnancy and was highly correlated to the pre-pregnancy BMI (1. Gale CR, Javaid MK, Robinson SM, Law CM, Godfrey KM, Cooper C. Maternal size in pregnancy and body composition in children. The Journal of Clinical Endocrinology & Metabolism. 2007;92(10):3904-11. 2. Ricalde AE, Velásquez-Meléndez G, Tanaka ACdA, de Siqueira AA. Mid-upper arm circumference in pregnant women and its relation to birth weight. Revista de saude publica. 1998;32:112-7).Therefore, in our study we have used criteria of either of BMI ≥25 Kg/m2 or MUAC of ≥ 28 cm indicated overweight and/or obesity.

Comment 7: Page 9, line 54, the reviewer disagrees with the sentence "low heterogeneity was observed on the previous history of GDM". Indeed, heterogeneity is significant for this analysis

Response: We agreed on your comment. We have corrected it based on your comment. We have checked heterogeneity was observed on the previous history of GDM (I² =64.4%). (see page 11, line 8 )

Comment 8: Table 2: subgroup analyses: it should be interesting to test for intergroup difference (between group heterogeneity and solely within group heterogeneity).

Response: The comment has been accepted. We have added column to heterogeneity statistics (among-study variance (tau-squared) for random-effects meta-analyses, degree of freedom and the I² statistic. (see Table 2)

Comment 9: For figures: please indicate the type of ES (prevalence for Figure 2 and OR for all the other figures).

Response: The comment has been accepted and corrected. We have edited and used prevalence instead ES for figure 2 and OR instead of ES for other Figures. (See Figure 2-5)
Comment 10: The reviewer strongly encourages the authors to perform subgroup analyses regarding study design. The high level of heterogeneity could have been brought by adding in the same meta-analytic model cross-sectional as well as prospective studies.

Response: The comment has been accepted and corrected. We have performed the sub group analysis by study design and the result has presented in the result section. (see page 9, line 18 -19 and Table 2)

Discussion

Comment 1: Heterogeneity is very important across all analyses, even in the subgroup analyses. This should be discussed as a limit of the manuscript.

Response: The comment has been accepted and we have put as a limitation of manuscript. (see page 14, line 13 -14)

Comment 2: Please explain the strengths of the manuscript.

Response: The comment has been accepted. We have mentioned the strengths of the manuscript as the pooled prevalence of GDM noted using the updated and current international diagnostic criteria and using similar definition of GDM allows to determine the current and true prevalence of GDM in Africa. Subgroup analysis (sub regions of Africa, years of study conducted (data collection period), risk of bias and study design) and assessed multiple factors were considered as the strength of our study. (see page 14, line 9-13)

Reviewer #2 Comments

Comment 1: Some sections, like the abstract, are too long and the authors must fully follow the instruction to authors of the Journal

Response: The comment has been accepted. Revision was made based on your suggestion and based on the journal of archive of public health recommended.
Comment 2: In the background, there are too many references regarding the inconsistent findings noted among studies conducted in Africa.

Response: We agreed on your comment. Revision was made based on your suggestion and used the most relevant references. (see page 4, line 18-19)

Comment 3: The rational for having only published articles from 2013 to 2018 must be explained.

Response: We found out that this similar comment raised by reviewer 1.

As we have respond for reviewer 1,

In 2013, WHO revised its recommendations for diagnosis of GDM taking into cognizance the issues raised by the International Association of Diabetes in Pregnancy Study Groups (IADPSG) recommendations. The WHO 2013 modifications along with common diagnostic criteria for GDM by 2013 American Diabetes Association (ADA). Though, in the last five years the diagnostic criteria have been changed and there is no known overall prevalence and associated factors of GDM after the change in Africa. Moreover, as clearly we justified the prevalence of GDM is not only affected by the population characteristic but also by screening and diagnostic criteria. We are interested to identify the pooled prevalence by updated or current diagnostic criteria which is recommended by WHO 2013 and IDASPG and ADA. African countries practices this criteria at this time. Therefore we are confident enough we point out the true and current prevalence of GDM by using similar definition of GDM. (See the title “Based on the Updated International Diagnostic Criteria”, Abstract section (page 1, line 3-5), Background section (page 4, line 5-24) and Method section (page 6, line 4-5 and line 19-23))

Comment 4: Sensitivity analyses must be performed according to the study design and the definition of GDM.

Response: We agreed on your comment without any reservation and sensitivity analyses has done to study design. We found no any influential studies that affect the overall estimate by study design. Moreover subgroup analyses has performed to study design and added its finding with the result section. Although we do respect your comment regarding the sensitivity analyses by definition of GDM; we have used similar diagnostic criteria for GDM (similar definition of GDM) as the beginning or inclusion criteria by using the current diagnostic criteria (WHO 2013). Additionally, there is no any entire classification of GDM. Therefore, sensitivity analysis for definition of GDM has done similar to sensitivity analysis for the prevalence of GDM. (see table 2)
Comment 5: Part of the manuscript is in bold without any rational.

Response: We have accepted this comment; and the background section has been revised by added more sentences of rational of study as you suggested. (See page 4, line 7-24)

Additionally, all other technical issues, sentence and grammar errors have been addressed.

Thank you for your valuable comments!

The Authors