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

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Author’s response to reviews:

Point by point response to reviewers

Dear reviewers,

First of all, we would like to thank for your generous support in improving the quality of this manuscript. We learnt a lot from your constructive comments. We have exerted our maximum effort to address all comments given by the reviewers. The revisions we made are presented as follows;

Reviewer #1 Comments

Abstract

Comment 1: please do not report all the OR and 95% CI in the abstract. Abstract is too long.

Response: We have accepted your comment and all the OR and 95% CI for factors has been removed in the abstract section. We have revised the abstract to make short as per your suggestion and based on the journal style recommended. (see page 1, line 20-21)

Methods
Comment 1: Authors searched in Pubmed and Medline database. Authors should be aware that Medline is a bibliographic database that can be accessed via PubMed interface. There are not two distinct databases.

Response: We have accepted your comment and corrected by using PubMed database instead of mix-up of Medline with PubMed. (See in method section page 5, line 9-10 and abstract section page 1, line 7-8)

Comment 2: Authors did not used MeSH terms in their search strategy. This should be reported as a limit of this study.

Response: The comment has been accepted and as you suggested we have reported as a limitation of this study. We thought all terms or phrase that could explain GDM and other important terms are exhaustively listed in the search strategy as we have stated in the manuscript (method section, page 5 line 10-20) and we have used a combination of expanded search term and free-text searches with the names of all countries included in Africa and sub-region of Africa as shown in Appendix 1. However, there could be uncommon terms of GDM that used by others scholars. Therefore We have accepted the comment and put as limitation of study as we did not used MeSH terms in their search strategy (see page 15, line 1-2)

Comment 3: Authors did not investigate grey literature. This should also be reported as a limit of this study.

Response: The comments has been accepted and we have reported it as limitation of study.(see page 15, line 1-2)

Comment 4: Authors reported to have used JBI tool to extract data. This is confusing. Authors should be clearer by explaining that data were extracted on an excel sheet (by the way, did the authors performed a pre-test for this extraction sheet?) and that they assessed for study quality using the JBI tool. JBI tool has been developed to assess risk of bias. Is it not a tool designed for data extraction. Moreover, how did the authors categorize the studies as "low, moderate or high" risk of bias? The cut-offs used should be reported in the methods section.

Response: Sorry for the confusing statement about our data extraction and quality appraisal. Revision was made based on your suggestion. Our interest was to mention the items in the appraisal tool that we used to evaluate papers but not the items we used to extract the characteristics of each papers (eg. Year of publication, sample size, design, study area, prevalence etc…). The authors (Hoy et al.’s risk of bias tool) adapted the JBI and others which
this quality appraisal tool addresses external and internal validity. Therefore to avoid this confusion we have modified the whole paragraph about quality appraisal. We have also added the reference and the items of the tool as appendix (see appendix 2a) and in addition to previously attached the risk score (value of risk of bias) for each study (see appendix 2b). In order to make this sentence clearer regarding to quality appraisal, it is revised as follows “The study quality or risk of bias was assessed using the adopted a risk of bias tool developed by Hoy et al. (35) and modified it to suit to our study. The tool consists of ten items that assess sampling, attrition, measurement and reporting bias. The validity of methodology, appropriateness and reporting of results were also assessed (Appendix 2a). When the information provided was not adequate to assist in making judgment for a certain item, we agreed to grade that item with a ‘NO’ meaning high risk of bias. Each study was graded depending on the number of items judged ‘YES’ as low ( ≥ 8), moderate (6 to 7) or high risk of bias ( ≤ 5) (Appendix 2b)”’. (See page 7, line 2-9).

Regarding data extraction, the selected papers were fully reviewed and the required information for the systematic review was extracted and summarized using an extraction table in Microsoft Office Excel software. We have checked (pretested) the excel sheet for the consistency and prepared under subheadings (name of author (s), country and sub-region, study design, setting, year of publication, year of study conducted (year of survey) , sample size, response rate, gestational age when GDM screen, participant selection, prevalence of GDM (including percentage and 95% CI), odds ratio of certain risk factors etc…). We have also 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. (See page 7, line 10-19)

Comment 5: Authors concluded about heterogeneity in their MA by using the I². The I² is a value that quantifies heterogeneity, but the presence of a significant heterogeneity should be done with Q test and its respective p-value. Moreover, table 2 reports Q-value but Q-value is not explained in the methods section.

Response: We have accepted the comment and revision was made based on your suggestion. We have included and explained Q-value in the method section to explain the presence of a significant heterogeneity done by Q test and its respective p-value < 0.05 in the method section. (See page 7, line 22-24) and abstract section (see page 1, line 10-12)

Comment 6: The choice of using a random effect model should not be based on the results of heterogeneity. It should be done in the protocol based on an estimate of a potential heterogeneity across results.
Response: The comment has been accepted. We have revised the sentences to make clear as follows “The heterogeneity was presumed in the protocol based on an estimate of a potential variation across studies and depicted in the analyses, we used a random effects model as a method of analysis.” (See page 7, line 24-26)

Comment 7: The protocol in PROSPERO did not specify the subgroup analyses that authors have performed on year of study, quality of study and study design. Are these analyses post-hoc analyses? This should be reported in the discussion section.

Response: To be honest, we added each subgroup analysis after identified heterogeneity analysis. However, after we get your valuable comment we learnt that we should make clear this analysis were of course post hoc analyses. Revision was made based on your suggestion and we have reported as it was post-hoc analyses in the discussion section. In addition if you recommend we can edit our protocol in the PROSPERO and mention each sub group analysis. (See page 12, line 26-28)

Comment 8: For each trim and fill analyses, authors should explain that results have been influenced or not by publication bias and explain that, by adding x studies, the pooled estimate varied to … (95 % CI …). Moreover, results of the trim and fill analyses should be discussed in the discussion section.

Response: The comment has been accepted. Revision was made based on your suggestion. We have checked the results influenced by added studies in the trim analysis and put the varied to the value from the pooled effect size before trim and fill analysis. We have also discussed about the variation in the discussion section. (See for example the result section ( page 9 line 3-4, page 10 line 1-3 and line 8-10, page 11 line 21-23 ) and discussion section (page 12 line 9-10 and page 14 line 11-13).

Comment 9: Page 10, line 14, authors reported that for figure 3, the sensitivity analyses showed no influential study that caused variation. What about the study of Sagheer and Hamd that reported an OR of 67? For more transparency, we encourage the authors to add a column to table 3 and report the range of results obtained with the one-study removed sensitivity analysis.

Response: We have re-checked the study about the non-influential status of articles study of Sagheer and Hamd that reported an OR of 67. As you saw in the forest plot it has no influential study that caused variation and the sensitivity analysis it was confirmed it has no any influence. Since the OR is unusual, we tried to review the article and we reach on the conclusion that it has no any problem and the sample size seems significantly rigorous. Furthermore, in the sensitivity
analysis there was no a single study its point estimate beyond the confidence interval. Therefore we had no any evidences to remove this article from the analysis. Having this excluding studies has no significantly changed the estimate in our analysis. We also checked the study removed sensitivity analysis. Still it has no influence.

As you recommend we have added a column to table 3 and report the range of result of by omitted one study by sensitivity analysis for each factors. (See table 3 for added column)

Comment 10: Table 2. Authors are encouraged to add in this table a p-value for interaction between groups.

Response: To test for group differences, the difference between subgroups was carried out by weighting of the effects size (i.e., GDM prevalence) while considering variance overall, within the subgroup and between subgroups. And this revealed consistent findings with the main analyses for GDM prevalence among subgroups. We face a challenge to put p-value for interaction between groups in Stata 14. We check it repeatedly but always the Stata output showed “Note: between group heterogeneity not calculated.” Any recommendation is highly accepted to get the put p-value for interaction between groups. Though the point estimate or prevalence in the subgroup analysis showed there is difference, but there was some overlap of CIs in the result of sub group analysis. This indicates these differences may not statistically be reliable as the CIs overlap. To make clear we included the following statement in the discussion section as follows “however still significant heterogeneity was also found in sub group analyses and these differences between groups may not statistically be reliable as the CIs overlap. This was further augmented by further supplementary analysis that revealed non-significant group differences.” (See page 13, line 7-10). Furthermore, some estimates could be influenced by an interaction between groups we have stated this as a limit of study. (See page 15, line 3-5).

Comment 11: Figure 3 should be formatted to be more readable.

Response: The comment has been accepted. Revision was made based on your suggestion to be figure 3 more readable. (See Figure 3)

Discussion

Comment 1: In the prevalence MA, a very large heterogeneity is still present in the subgroup analyses. This should be discussed.

Response: The comment has been accepted and revision was made based on your suggestion. We added more sentences about subgroup analysis in the discussion section (see page 12, line
26-30, page 13, line 1-10), again as we stated in the previous response for comment 10 the presence of high heterogeneity in the subgroup analyses mentioned as a limit of study. (See page 15, line 3-5).

Comment 2: Some studies in figure 2 report a very large prevalence of gestational diabetes mellitus. These findings deserve to be discussed.

Response: The comment has been accepted and we have added sentences to discuss some included studies in MA with a high prevalence of GDM. (See page 12, line 10-14).

Reviewer #2 Comments: ok

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

Thank you for your valuable comments!

The Authors