Reviewer’s report

Title: Gestational diabetes and pregnancy outcomes - a systematic review of the World Health Organization (WHO) and the International Association of Diabetes in Pregnancy Study Groups (IADPSG) diagnostic criteria

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Reviewer: Shareen Forbes

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Other comments

Strength of paper
1. exclusion of studies conducted with selective screening.
2. Studies not allowing for analysis of untreated women.
3. search strategy
4. methodological rigor in excluding IADPSG
5. availability of the EBDG study, a relatively large Brazilian study, to generate data when other studies were not available from the published literature

Other minor comments

My comments are in the order that they appear in the manuscript:

1. in the abstract it would be better to state that two criteria exist. Rather than the statement “…..are being recommended.” The WHO criteria were never based on pregnancy outcomes whereas the IADPSG are based importantly on pregnancy outcome data. The authors could be more clear re this.

2. in the results in the abstract the authors describe pregnancy related hypertension with respect to the WHO criteria and pre-eclampsia with respect to the IADPSG. Is this correct? Definitions are not described in the text.

3. in abstract again to what extent do the authors feel that the performance of the IADPSG criteria are reduced in the context of reducing the numbers examined by over 50% and hence by reducing the power of the study ? in the main text and table 1 it is clear that when HAPO is excluded 21513 subjects remain.

3. in the background 4th paragraph 2nd line “glucose intolerance” should be changed to impaired glucose tolerance. The fasting definition for the WHO for diagnosis of GDM should also be noted (>7mmol/l)

4. I wondered whether the inconsistencies viewed across the studies are secondary to the very different ethnic groups included. It is well known that there are differences in birth weights according to ethnicity. Are the authors able to comment for example on the results in this context and do they change in this context. If they cannot report on this they should discuss that this may contribute to the inconsistencies between studies which I note include studies from Pakistan
and Japan.

5. the authors point out quite rightly that it is too early still to fully evaluate IADPSG diagnostic criteria. This point is well taken.

6. discussion. 2nd paragraph – better to state that the IADPSG have lower glycaemic thresholds rather than state that it is a milder form of GDM.

**Declaration of competing interests:**

I declare that I have no competing interests.