Reviewer's report

Title: Glucagon like Peptide Analogues for Type 2 Diabetes Mellitus: Systematic Review and Meta-analysis

Version: 2 Date: 4 November 2010

Reviewer: Olivia J Phung

Reviewer's report:

General Comments
1. The revision is improved from the original submission. However, there are some points that were addressed by the authors only in the response to comments document, but no revisions were made to the manuscript when it would have helped to clarify statements made.

Minor Essential Revisions
2. The thresholds for the interpretation of I2 are not mutually exclusive and there is considerable overlap between categories. Please clarify how an analysis falling in two categories would be assigned.

3. I am unclear how the authors arrived at mean age, baseline A1c, etc for their study characteristics. Did the authors meta-analyze this baseline data in Review Manager and report it here? Reporting the mean number of participants in each trial seems flawed; it can only be calculated from a simple average and may not be representative of the data. Why not a median?

4. The rationale given for excluding Rosenstock 2009 from the analysis on the basis of metformin is inadequate. Needs some elaboration on why inconsistent metformin use would affect study results. Also, there needs to be a statement of whether this was an a priori exclusion criteria in the Methods section or a post-hoc decision. Otherwise, it appears that this study was singled out.

5. In both the Results and Discussion sections: The author responses state that weight loss being independent of nausea was a stated result of a study and no numerical data were available. If this is the case, please cite accordingly and qualify statements that no numerical data were available. Otherwise, this claim is unsupported as it stands in the manuscript.

6. Discussion: Authors state that exenatide 10 mcg bid was equivalent to insulin and rosiglitazone. The author’s response to the comment adequately addressed my concern, but no clarifications to the manuscript were made. The authors appear to base equivalence on a clinically relevant A1c threshold of 0.5%. This information should be added to the Discussion section with a reference to a source that supports that threshold.

7. The authors state that the incidence of hypoglycemia was low with GLP-1 agonists, but did mention what the comparison was to judge low. The response to comment states that this is low in absolute terms. This then needs to be
qualified in the manuscript, by stating that the absolute incidence is low. Otherwise, “low” itself is a relative term.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests