Reviewer’s report

Title: A Randomized Controlled Trial to Investigate the Impact of a Low Glycemic Index (GI) Diet on Body Mass Index in Obese Adolescents

Version: 1 Date: 12 December 2013

Reviewer: Angeliki Papadaki

Reviewer’s report:

This is an important paper examining the impact of a low glycemic index diet in an Asian population of obese adolescents. The findings of the paper are of major importance considering the prevalence of type 2 diabetes in China. The authors clearly present earlier research and how their study builds on that, as well as the novel aspects of their study, which is commendable (e.g. the consideration of physical activity levels and energy expenditure). Although the research question raised is valid and appropriate methods were applied to answer it, I believe that more data on actual compliance with the GI component of the intervention are needed to support the authors’ conclusions.

Major compulsory revisions:

1. Some editing with regards to English language would be needed to improve comprehension, and I suggest the authors have someone whose first language is English proof-read their manuscript.

2. Methods – Interventions, 2nd paragraph: Please provide information (earlier than this section) on how desirable weight status was defined for each subject. Later on you mention that a 20% caloric reduction was applied. However, some might argue that there are issues to consider when applying weight loss diets to children and adolescents, since that might affect their growth. In fact, a common practice for weight loss in this age group is to increase physical activity (and therefore energy expenditure) and focus on improvement of eating habits rather than caloric restriction. Can you please clarify/discuss this? Also, in the 3rd paragraph, please define low-calorific products.

3. Methods – Interventions: The information discussed in the counselling sessions seems really complicated to be explained to parents and children in either 20 or 30 minutes.

4. Results: As this was an intervention promoting a low-GI diet, the results (e.g. in Table 2) should contain information on the baseline and post-test GI of the adolescents’ diets in the two groups. This is important to examine compliance levels with the study’s intervention but also to examine whether it was indeed the low-GI advice provided to the intervention group that caused the favourable changes in the intervention group. This is needed before the authors can reach the conclusion that ‘The results of our study provide additional evidence and important insights regarding the role of low GI diet in obesity management of our
youth populations’.

5. Discussion - 5th paragraph (limitations): ‘This may account for the insignificant differences in carbohydrate intake between the adolescents in the control and low GI group’. Although this is potentially a valid point, these questionnaires did manage to detect differences in fat, energy, protein and fibre intake, so this reviewer does not understand why differences in carbohydrate intake might not have been detected as well. Nevertheless, and similar to my previous comment, the effect of the intervention on GI values would be more important to present here.

6. Discussion – 5th paragraph (limitations): ‘Third, our study captures outcome measures at 6 month which may be too short to detect any significant changes in lipid profile and glucose intolerance’. I agree with this argument, however a longer intervention might further increase attrition rates. Please discuss this.

6. Discussion – last paragraph (conclusion): Please tailor your conclusions to the methods applied. This was not only a low-GI intervention, since there were other intervention components as well (e.g. weight loss). For example, the DiOGenes study involved an ad libitum energy intake, whereas the present study was a weight loss study promoting a low-GI diet. Therefore it might be difficult to distinguish whether the effects observed were due to the low-GI advice provided or the reduction in energy intake that was promoted (especially since no data on GI are reported). Please acknowledge this.

Discretionary revisions:

1. I recommend that the authors consider also presenting intention-to-treat analyses (in addition to the per protocol analyses), to account for the high attrition rates that occurred during the study.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

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