

Author's response to reviews

Title: Lowering the Glycemic Index of White Bread Using a White Bean Extract

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Lowering the Glycemic Index of White Bread Using a White Bean Extract
Jay K Udani MD, Betsy B Singh MD, Marilyn L Barrett PhD and Harry G Preuss MD

Dear Nutrition Journal,

We thank you for the reviewer's comments on our manuscript. We would now like to resubmit our manuscript that has been revised in accordance to those comments. Below is a compilation of the comments from the 3 reviewers along with our responses.

Thank you for your consideration of our revised manuscript.

Suggested Edits:

Comment: Page 6 Line 10. "Patients" must be "Study volunteers" or "Subjects". In this kind of trial study, patients with some ailments shall not be used.

Response: Change made.

Comment: Page 4: The statement that "GI is more reliable...as it can predict the response in any individual" does not seem quite accurate. The evidence is that GI predicts the average response among individuals, with a fair amount of variability between individuals (as evidenced by the wide standard deviations in table 1).

Response: Statement removed.

Comment: 8. On page 7 the authors state that fat does not affect the GI of foods, citing reference 19. This is not what was found in reference 19 and there is plenty of evidence showing that fat does lower glycemic response. However, since fat was used both with the control and the test breads the work by the authors is internally consistent.

Response: Sentence changed.

Comment: 9. There is repetition of the statistical analysis sentence on page 8.

Response: Repetition removed.

Comment: 15. The Abstract needs to be amended to take account of the preceding points.

Response: Changes made

References

Comment: 11. On page 11, references 29 and 31-35 are being cited. My copy of the script only has references up to 25. The literature cited is problematic in other places as well. In some cases the statements do not seem to be supported by the referenced papers. There are also references to sources 31-35 which were not included in the review version of the paper.

Comment: Page 4: The authors should check the references for the final sentence of the first paragraph. The papers cited do not appear to support the statements on cholesterol and decreased body weight in adolescents. Also epidemiologic evidence seems slightly overstated based on the papers cited.

Response: Apologies for the references. They have been reviewed and updated.

Comment; I am curious about the "Phase 2" supplement. The authors state that it has been shown to be an alpha-amylase inhibitor, but the reference they cite is about a polypeptide

isolated from a Streptococcus species. Is there a description of the active ingredient(s) of the supplement?
Response: Reference changed.

Comment: Page 10: The lack of blinding of participants and of study staff should be acknowledged as a limitation.

Response: Done.

Comment: Page 6: Were the test meals administered in a standard order? Random order? How far apart in time were the measurements? This could be important as there could be seasonal differences in GI response.

Comment: 5. The authors should state whether the order of consuming the test breads was randomized.

Response: Information added to paper. Meals were administered in a standard order. The measurements were made within 2 weeks.

Comment: 4. State when the capsules were taken.

Response: Information added to paper. Capsules were taken immediately before meal.

Comment: 10. On page 5 it is said that the objective was to lower the GI of a 'high glycemic' food product. 'High' has a particular meaning when related to GI (GI > 70) and since the control bread had a GI of 59 this could be confusing to some readers.

Response: The sentence in question is: "The objective of this study was to determine whether Phase 2 could lower the effective GI of a common high glycemic food product." White bread is considered a common high glycemic food even though in this experiment it had a lower GI.

Comment: 6. Were the test breads all from the same batch?

Response: Information added to paper. Purchased at the same time.

Comment: 7. In the Methods it says that 16 people were randomized and 13 were eligible. This is not consistent with the Results in which 15 subjects began. Explain the rationale for excluding two people whose blood glucose exceeded 200 mg/dL to the reference glucose beverage.

Response: 15 people were randomized. Information changed in paper.

Comment: 13. Care should be taken not to overstate the benefits of GI. Study findings are not consistent (Am J Clin Nutr 2008;87:114-25, JAMA 2008;300:2742-53) and there are attributes to these diets other than low GI that could contribute to their effects. The foods used in these low GI diets tend to be minimally refined rather than refined products engineered specifically to have a low GI. Engineered or supplemented foods may work as part of a low GI diet but this is untested. The FAO/WHO are cautious on this (Eur J Clin Nutr 2007;61 suppl 1:S100-111).

Comment: 14. On page 11 the authors are suggesting that decreasing GI may contribute to weight management. The role of GI in weight management is not clear (Eur J Clin Nutr 2007;61 suppl 1: S75-S99). A better argument, if the authors can substantiate it, may be that starch blockers inhibit glucose transfer across the intestine thereby reducing the usable energy content of the food.

Response: The discussion has been edited in response to these comments.

Comment: It is helpful to know whether the white bean extract is odorless or tasteless in case of additives to high GI foods. Are there data on taste/palatability of the supplement in powder form? This could have a major impact on the use of the supplement.

Response: Information added to paper. Odorless, tasteless

Statistical inquiries:

Comment: Page 8: It is not clear whether the questionnaires on gastrointestinal symptoms asked about only the 2 hour GI test period. I would think the full day would be important.

Response: Only 2 hours were tracked.

Comment: Page 8: Why was ANOVA used if the reported results were t-tests? Observations under different conditions in the same individuals will be correlated. It is not clear whether this was accounted for in the analysis (eg by using paired t-tests or ANOVA accounting for repeated measures within subjects).

Response: Followed WHO guidelines for calculating GI

Comment: Page 8: It is not clear what “triangle A”, “trapezoid B”, etc refer to. There could be a figure which labels these shapes or the method could be described in a different way.

Response: Section removed from manuscript.

Comment: P-values are generally only reported to 2 digits after the leading zeros.

Response: P-values changed.

Comment: 1. The authors are claiming a dose response. What statistical test was applied to test this?

Response: There was the appearance of a dose related effect but no statistical test was conducted. The sentence in the text has been modified.

Comment: 2. Explain in the statistics section the reason for conducting unadjusted multiple t test comparisons. There are more conservative adjustments that can be applied when multiple treatments are compared against one control.

Response: Followed WHO guidelines for calculating GI

Comment: 3. Put the power calculation in the statistics section: if there was no power calculation, explain why. The authors state that the small sample size was a limitation. Large variability in GI is known (Br J Nutr. 2008;100:364-72).

Response: No power calculation was done. The GI testing was completed with 10 subjects according to WHO protocol and this information was added to the manuscript.

Comment: 12. The terminology around what is and what is not a reduction needs attention. The only reduction found was with the 3000 mg powder formulation. Other powder doses or capsules may have the potential to lower GI but this was not shown, possibly due to insufficient power. In the Conclusions the authors state that ‘All other dosages/formulations (except the 1500 mg capsule form) showed clinically meaningful reductions without reaching statistical significance’. This should be reworded to reflect the uncertain nature of the findings ie: that if the differences were real, they could have clinical meaning. This is important because the practicality and affordability of using this bean extract will depend on the amount needed. If doses lower than 3000 mg are desirable this will need to be tested with a sample size sufficiently powered.

Response: Document modified as suggested.

Comment: Minor Essential Revisions 2. Adding a Figure of the incremental areas under the curves would give a better impression of the effect of each of the treatments.

Response: Comment noted. We hope that the current data will suffice and have presented it along the lines of other published literature.