Reviewer’s report

Title: Comparison of high protein and high fiber weight-loss diets in women with risk factors for the metabolic syndrome: a randomized controlled trial

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Reviewer: Kelly Meckling

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This is an interesting manuscript that seeks to determine whether, in principle, higher protein diets have benefits not observed in higher carbohydrate, high fiber diets for weight loss and MetS risk reduction. Overall the study is well designed, the methods and assays appropriate and the general conclusions supported. There are however, a few criticisms of the manuscript as currently presented that the authors/editors should consider as revisions before publication. In particular the authors refer to "marked differences" between diets where the statistics do not support such statements.

Compulsory Revisions.

1. The title of the manuscript suggests that a significant portion of the study population have MetS. In fact, only 9 or 8 (of 41 or 42) subjects in each group met the IDF criteria for MetS and similar numbers for insulin resistance. This suggests that the majority did not have MetS. It would be more appropriate to describe the cohort as an overweight population with abdominal obesity. The background suggests that this population is particularly at risk of developing MetS but it is not clear what the criteria for this "predisposition", other than central adiposity, are being used.

2. In the results section, paragraph 2, the first sentence suggested that "completed diet records" were returned. In the methods section 3 day weighted records were completed. Is this what is being referred to as "complete diet records"? To this reviewer, complete diet records suggests food diaries were kept throughout the 8 week intervention. The nature of the records should be clarified.

3. In addition to differences in protein and dietary fiber intake, there was also a lower fat intake in the high fiber diet. This is not acknowledged in the results section and is an important consideration. It would also be helpful to indicate what the expected weight loss would have been based on the reported energy deficit relative to what was actually achieved. My crude calculation suggests that the HP group lost an addition 1.5 kg that could not be explained by the level of energy restriction.

4. The results state that the decreases in blood pressure were greater on the HP diet. This is only statistically significant, however, for the diastolic blood pressure. Also for several of the blood measures (i.e. fasting plasma glucose) the results text suggests "reductions" but in the data table these "changes" are not
significant. The statistics in the table(s) only refer to differences (if significant) between diets and not whether there is a time effect (of either diet). For example the starting plasma glucose in the HP diet is 5.0 mM and is 4.9 mM, 8 weeks later; is this difference significant? It is seems unlikely. This needs some clarification for all of the biochemical measures or calculated values (McAuley, HOMA).

5. In the discussion section there are several additional studies where protein and carbohydrate level were examined, that are missing from the list of references. This is particularly true for the hypocaloric HP diets where the authors suggest there are only two such studies; there are several more.

6. In the discussion the authors suggest that the difference in blood pressure reduction between the HP and HFib diet could be due to the higher sodium consumption in the HFib group and the "increase in fluid retention" that could occur. This seems highly unlikely since both groups were consuming sodium levels well below recommendations and both were consuming less sodium that they were on basal diets.

7. Page 15, paragraph 2; the conclusion is that favorable changes in lipids and insulin sensitivity was more "marked" in the HP group. The authors suggest that "conventional statistics" do not support this. Either the differences are significant or they are not.

8. The authors should consider the fact, that substantial changes, and differences between diets may not have occurred because the population under study had "normal" values to begin with. An insulin value of 9 is normal so one would not really expect a decrease. Those with abnormal values may have changed but the differences were diluted by the many subjects who started out with normal blood chemistry.

**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.