Author's response to reviews

Title: Restriction of meat, fish, and poultry in omnivores improves mood: a pilot randomized controlled trial

Authors:

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Author's response to reviews: see over
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Dr. Nehme Gabriel
Editor-in-Chief
Nutrition Journal

Dear Dr. Gabriel:

Please accept resubmission of our manuscript entitled, “Restriction of meat, fish, and poultry in omnivores improves mood: a pilot randomized controlled trial ” as a Short Report in your journal.

Thank you for granting extra time to do our revisions, although we were able to complete them within the original requested time frame. Our responses to the referees are below:

Response to Referees– referee comments are italicized and authors’ responses are bolded to facilitate reading.

Response to Referee 1

Discretionary Revisions

I would suggest changing the words “omnivore diets” to “omnivorous diets”.

“Omnivore diets” has been changed to “omnivorous diets”.

It is helpful to use no more abbreviations than necessary. I would suggest not abbreviating “meat, fish, and poultry”.

The words “meat, fish, and poultry” are now spelled out consistently.

Page 3: The heading “Findings” appears to be in error. I presume this should be labeled “Introduction”.

This manuscript has been proposed as a Short Report. The ‘Guide for Authors’ shows “Findings” as the only major heading up to the Conclusions, however, we missed the note that minor subheadings could be used, and have added them.

Page 3, line 3: I would suggest changing “Diets rich in seafood are an exclusive source of the anti-inflammatory long-chain n-3 fatty acids” to “Fish and shellfish are among the few dietary sources of long-chain n-3 fatty acids.” Long-chain n-3s have also been identified in seaweed, which is a nutritionally significant source for some populations.

The sentence has been changed as suggested.

Page 3, line 10: I suggest you qualify this sentence suggesting that vegetarian diets are low in n-3 or n-6. They may be in some cases, so it would be important to identify the source of these data. However, if the diet includes soy products, walnuts, and flax, the ALA content will be relatively high. Also, n-6 oils may be found in many common cooking oils.

We agree; the sentence has been qualified by inserting the words “long-chain”.

Page 4, lines 12 and 21: I would suggest changing the ambiguous word “restrict” to the more explicit word “avoid”, if that would be correct.
Yes, it would be correct, and the word has been replaced in this context.

Page 5, line 21: It is noteworthy that the n-6 to n-3 ratio in a vegetarian diet can vary dramatically, depending on the foods chosen. Greens have a much more favorable ratio, for example.

There does not appear to be a revision to be made here.

Page 5, lines 23-25: I would suggest moving the discussion of fatty acid conversion to the discussion section.

The text regarding fatty acid conversion was moved to the end of a similar discussion two paragraphs later.

P. 7, lines 1-2: I would delete the suggestion that humans “evolved on a hunter-gatherer diet.” Such an era was only one phase of human evolution. You may wish to look at D.J.A. Jenkins work on the “simian diet,” in which he argues that early human diets were more like those of other great apes and that substantial increases in meat-eating were a later phenomenon. Similarly, Richard Leakey’s team showed that meat-eating probably began as scavenging, but not until humans had developed stone tools, suggesting that people were essentially herbivores for much of our pre-Stone Age history.

We agree, and our statement has been restated a bit, and the revised statement now begins the following paragraph.

Page 7, last paragraph: The “limitations” you mentioned are actually possible mechanisms that would explain your findings. It is good you’ve mentioned them, but I would take them out of this paragraph and describe them earlier.

Agreed - the first ‘limitation’ regarding vegetarian diet and antioxidant protection may be a mechanism and has been moved to the end of an earlier discussion paragraph.


I am familiar with the 2003 paper, but was unaware of the 2010 paper at the time of manuscript preparation. I have inserted a reference to the Welch et al article in the limitations paragraph.

Page 8, line 4: The suggestion that “U.S. omnivores who will likely never adopt a vegetarian diet....” is overly pessimistic and does not follow from your results. It should be omitted. If the findings of your study hold true in further studies, then people who are troubled by persistent mood disorders may well wish to try a plant-based diet, as an increasing number of people are now doing for other reasons. After all, depression is treated with drugs that have many side
effects, as well as electroconvulsive therapy. In comparison, a diet change seems quite benign. The acceptability of plant-based diets in clinical research has been shown to be on a par with other therapeutic diets. For example:


We appreciate these insights; the concluding statement was omitted.

Table 2. I would suggest adding descriptors to the scales to facilitate interpretation.
Descriptors have been added to both scales.

Thank you for the opportunity to read this important and interesting manuscript.
Thanks for your very astute and helpful suggestions!

Response to Referee 2

This is a clinical trial to investigate the effect of three different dietary patterns (omnivores, vegetarians, FISH (consuming fish but restricting meat and poultry) on mood. Before further consideration, reviewer suggests that authors should address the major critiques listed and numbered below:

1. First of all, the format of the paper is different from most of original research articles. This manuscript has been proposed as a Short Report, hence the different format per Nutrition Journal. We ask the editors to please advise us regarding formatting if we have been remiss.

2. Obviously, this is an open trial and both the researchers and participants know which dietary intervention is being administered. Authors should provide information about how they prevented the occurrence of conscious and unconscious bias in the conduct and interpretation of a clinical trial arising from the influence which the knowledge of treatment may have on the recruitment and allocation of subjects, their subsequent care, the attitudes of subjects to the treatments, the assessment of end-points, the handling of withdrawals, the exclusion of data from analysis, and so on.

We appeared to intentionally focus on a more general role of protein in brain function in our study announcement to mask our focus on mood. Also, one of the participant protocol components at baseline and study end was a brief computerized cognitive test. This information was never tabulated as it was not the intent of the study and the measure was not validated. Additions were inserted into the methods text in two highlighted places.

3. Although this is a randomized trial, given the small sample size in each arm (n=13) reviewer suggests that authors should examine potential confounders were comparable in the three groups at baseline. If the distributions of potential confounders at baseline are significantly different between groups, multivariate analysis is necessary despite of randomization.

True; in the paragraph following the description of statistical analysis, the following was stated in regard to our analysis of potential confounders at baseline:
“Groups did not differ at baseline by age, gender, BMI, educational level, ethnicity, total physical activity level or fatty acid intakes. There was a significant difference between the OMN and VEG groups for the POMS-C score at baseline [median (IQR): 9 (8) and 3 (5) respectively; \( p=0.007 \)].”

4. In addition to within-group comparison data, authors should also provide data of between-group comparison.

We did provide data of between-group comparison in both tables as noted in footnotes b and c of Table 1 and footnote b of Table 2, but we have revised the footnote text to state this more clearly. These between-group differences were also featured in the results text regarding fatty acids and DASS scores.

As noted, we have incorporated the referees’ thoughtful suggestions and are confident that our manuscript has been further improved. Thank you in advance for your consideration.

Sincerely,

Bonnie L. Beezhold, PhD, CHES

Carol S. Johnston, PhD, RD