Author's response to reviews

Title: Does Pomegranate intake attenuate cardiovascular risk factors in hemodialysis patients? Results of a randomized placebo controlled trial

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Author's response to reviews: see over
Author's response to reviews (2)

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Title: “Does Pomegranate intake attenuate cardiovascular risk factors in hemodialysis patients? Results of a randomized placebo controlled trial” revised paper.

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Author's response to reviews:

Reviewer 1:
1. Minor Essential Revisions

1.1. In the introduced description of placebo juice: “The placebo contained: pomegranate artificial extract of Frutarom Ltd; Corrosive acid; Caramel as color material…” What do the authors mean by “corrosive acid”. There must be a typo!
Response: corrosive acid means "Citric acid"

1.2. It should read “chemistry” instead of “chimtry”
Response: Corrected

Reviewer 2:
1. Reviewer report:

1.1. Comment (#1.2 in the previous response): Page 5, second paragraph: “During the study period patients were instructed not to drink any other fresh fruit juice at home”. In this case, the patients assigned to the placebo group were at a disadvantage because they can not benefit from anti-inflammatory and antioxidant effects of these fruit juices. Therefore, is not surprising that in the placebo group, pulse pressure and plasma levels of triglycerides significantly increased (p=0.02 and p=0.04, respectively) and systolic blood pressure also
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changed to worse, although this increase was not significant (see table 1 and 2). I wonder if any of the statistically significant differences between groups might be explained for both factors: an improvement in the PJ group and an impairment in the placebo group. The authors should discuss this point in the text.

Response: We add this point in page number 9.

1.2. Comment (#1.4 in the previous response): Page 7: “Characteristics of the 101 patients are described in our previous paper [18]”. In my opinion, these characteristics must be expressed in detail, using a table, in the present manuscript.

Response: Selective demographic and other characteristics of the study population were detailed in the text (page 7). We believe that expressing the data with a table (already presented in our previous article) will create duplicity and will not be in line with keeping limited journal space.

1.3. Comment (#1.5 in the previous response) At baseline, the number of subjects with SBP> 140/90 mm Hg, those with HDL values < 40 mg/dl and those with triglycerides > 200 mg/dl were always higher in the PJ group (respectively: 26 vs. 5 / 54 vs. 23 / 20 vs. 10). Could this translate a defect in the randomization? In any case, this would facilitate positive outcomes in the group of PJ, since more ill patients, are also, patients which more can improve. The authors should discuss this point in the text.

Response: The analysis of patients with pathological levels, as detailed above, included only patients for whom no changes in the number of anti-hypertensive and lipid lowering medications were done during the study period. A stable medication therapy during the study, which was more pronounced in the PJ group as compared to the placebo group, was most probably due to the beneficial effect of the PJ. As for the comment regarding a possible defect in the randomization, we would like to remind that at baseline, no differences in the demographic and clinical parameters, including no difference in blood pressure nor in lipid profile, were noted between the two groups, indicating randomization success.