Reviewer's report

Title: Antioxidative protection of dietary bilberry, chokeberry and Lactobacillus plantarum HEAL19 in mice subjected to intestinal oxidative stress by ischemia-reperfusion

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Reviewer: Ignazio Castagliuolo

Reviewer's report:

This work by Jakesevic M. et al. address an interesting and modern topic: develop foods able to modulate the inflammatory processes in the host. Indeed the Authors use a relevant animal model and also attempt to determine the concentrations of anti-inflammatory and scavenger substances in their food supplement and, more interestingly, in the intestinal content.

The paper is well written and clear although the discussion appear quite long (six pages) and should be condensed. In addition the Results section in the abstract is not clear and a review could be helpful.

There some points that this Reviewer would like to point to Authors attention:

1. In the para “experimental diets” the authors indicate that the L. plantarum HEAL19 was mixed in freezing medium and then administered to the animals. Is this treatment able to preserve the probiotic alive or the bacteria are supposed to be dead in the food. If the Authors suppose to keep the microbes alive, did they test the vitality after 24 hours?

2. Can the Authors specify when the animal chow is prepared? Is it prepared daily with fresh ingredients and bacteria) or do they make a stock? In the latter case how is the vitality of the probiotic after 10 days? This point in important in view of the limited effects of probiotic supplementation per se or in combination: is it possible that the vital dose is lower than 10^8 cfu day?

3. In the para “experimental groups” please indicate for each group the treatment you used

4. Para “Malondialdehyde” the Authors should clearly state how they normalize the different samples.

5. In the results section, page 12 lane 4, the authors refer to “other abnormalities”: they should specify these anomalies since they discarded some experimental animals on these parameters.

6. the data regarding colonic tissue MDA should be reported in a more clearly, comparing the effect of I/R to sham and then the effect of treatment to I/R.

7. As the Author state also in their conclusions, would be helpful to few more measurements of the tissue damage and inflammation such as histology and MPO quantification.
8. How do the Authors explain the large variations in anthocyanin composition and concentrations, taking into account that mice are kept in similar conditions and receive an identical diet? How relevant is this aspect in light of administration to humans that already present an important variability in gut microbiota?

9. Is it possible to correlate the level and type of anthocyanin and phenolic acids formed from microbial degradation to the level of lipid peroxidation?

10. In the Para “conclusions” the authors indicate that chockberry did not have anti-inflammatory effects in their study, however, as stated above, the Authors did not measure any parameter of inflammation and this statement should be refrased.

**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.