

Reviewer's Comments	Responses
<p>Reviewer 00036318</p> <p>This is a well-conducted meta-analysis in an important topic. I believe that it would be more useful if the authors reported changes in bmi as kg/m<sup>2</sup> and in lipids as changes in mg/dl so that it is clearer if the effects are clinically meaningful. Please also add a paragraph with conclusions at the end of the discussion.</p>	<p>Thank you so much for the compliments. We are pleased to know that our work is beneficial and qualified. The changes in BMI and lipids were in the suggested units. The conclusions were added as part of the Article Highlight.</p>
<p>Reviewer 01809054</p> <p>The manuscript is current and the methodology is well done. The results and the discussion are good and the authors discussed the limitations of article.</p>	<p>Thank you very much for the compliments.</p>
<p>Reviewer 03478516</p> <p>Authors should add a recent systematic review dealing with both pre/probiotics in NAFLD, which reach the same conclusions of those of the authors as ..... Systematic review on intervention with prebiotics/probiotics in patients with obesity-related nonalcoholic fatty liver disease. Future Microbiol. 2015;10(5):889-902. doi: 10.2217/fmb.15.13. Authors should state that unfavourable articles are generally not submitted to any journal or worse not accepted for publication by quite all the journals. Very good the limitation about the lack of the eventual presence of side-effects.</p>	<p>We read the suggested recent systematic review with interest but it did not cover all of the three fat metabolism as presented in our work. Thank you very much for complimenting the limitation in the discussion.</p>
<p>Reviewer 03647461</p> <p>In the results section, it is assumed that “articles” and “studies” are used interchangeably to mean the same thing. Is that right? For example, you stated that the literature search removed “23 studies targeted irrelevant patient population,” ... Right after that was the following sentence: “49 studied focused on food-based probiotics and/or mixed probiotics”. Prior to that, you stated that “3,293 irrelevant articles were removed”. First, what is meant by “irrelevant population”? In your screening methodology, you consistently used the word “articles” in keeping or removing them from consideration in the</p>	<p>Yes, both terms were used interchangeably.</p> <p>Irrelevant patient population means patients with conditions listed in the exclusion criteria whereas irrelevant articles/studies were determined by the</p>

analysis. However, when it came to the above quoted “irrelevant population”, you cited 23 “studies” and not articles. Does this mean that the 23 studies that had irrelevant population target were also included in the “3,293 irrelevant articles” that were removed? Please clarify these statements. In this meta-analysis, there are several factors that are heterogeneous and do not follow a common modality in assessing the validity of the results from these various studies as a conclusive evidence of the effects of the probiotics on obesity, dyslipidemia or fatty liver disease. However, this study sheds some light on the contributions of the genus *Lactobacillus* more so than the other two genera, *Bifidobacterium* and *Pediococcus*, because of the limited number of studies generated from the latter two. Among, the primary outcome measures for fatty liver, obesity, and dyslipidemia listed in the Abstract was fibrosis score; and among the secondary outcome measures was subcutaneous fat. Both were not available in the outcome measures of the included studies that were reviewed. These measures would be critical in determining the three main disorders of fat metabolism, which lead to metabolic syndrome and NAFLD. This is important because probiotics as you correctly stated are one of the common ways to manipulate the gut microbiota as part of NAFLD management. Although, the comparison among the various studies was restricted to 12 or less number of weeks of probiotic administration, the duration of the benefit of the supplements to the users was not specified indicating that ceasing the intake of the probiotics after 12 weeks or less may not prolong the presence of the beneficial bacteria. As Table 1 shows, the sample size for each trial study is less than 130 people. The range of intake duration as stated on page 10 is not correct when compared to Table 1. Among the 13 included studies, the manuscript states that “six trials treated

screeners as irrelevant to the purpose of this review.

The fibrosis score and subcutaneous fat were anticipated when we developed the protocol but were not present in any of the selected studies.

We agree with the observation but believe that no changes to the manuscript are required for this point.

The range of intake was from 63 to 168 days.

<p>subjects for less than 12 weeks". However, in Table 1, only 4 were under 12 weeks, i.e. &lt;84 days. Also, eight and not six trials chose the exact 12 weeks for intervention. This numbers should be reconciled.</p>	<p>The numbers in Table 1 and 2 could not be directly compared as the subgroup analysis in Table 2 extracted more detail data from the selected studies.</p>
<p>Reviewer 00199807</p> <p>Dear Editor, I reviewed the manuscript titled "Probiotics for dyslipidemia, fatty liver, and obesity: A systematic review and meta-analysis". I think, there are some meta-analyses about this topic, and this manuscript does not add new information about this issue. This paper is not suitable for publication in WJG. My suggestions are listed below: 1. Title: The words "single strain" would be added to title as "Single strain probiotics for dyslipidemia, ..." 2. It is not clear the hypothesis of this meta-analysis. If single probiotic strain useful for dyslipidemia, is it cost-effective to use single strain instead of multiple-strain ones, in practically? 3. I think, it would be very useful if the Authors added clinical reflection and practical importance of these improvements of lipids in obese patients. 4. A conclusion paragraph should be added to the end of the discussion section.</p>	<p>Thank you very much for the advices.</p> <p>We agreed with adding the words "single strain" to the title.</p> <p>Cost-effective analysis is beyond the scope of this review.</p> <p>We believe that adding clinical reflection at this stage is still too early.</p> <p>The conclusions were added as part of the Article Highlight.</p>