Reviewer’s report

Title: IL-10/STAT3 is reduced in childhood obesity with hypertriglyceridemia and is related to triglyceride level in diet-induced obese rats

Version: 1 Date: 23 Jan 2018

Reviewer: Alan Remaley

Reviewer's report:

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Manuscript investigate in both a cross-sectional study of humans and in a rat feeding study the role of adipocyte derived IL-10 in modulating plasma lipids.

Specific Comments:

1. Abstract: More quantitative results from key finding should be included. Not sure what authors mean by "recognition markers". Conclusion sentence should be rephrased not in terms of a hypothesis but implications of main findings.

2. Line 32-37 is a run-on-sentence and should be reworded. Paper should also be carefully reviewed for other grammatical errors in text.

3. Methods: Some sort of power calculation or justification should be provided for sample size of clinical study and or state that this was an exploratory study. Exact BMI level to define obesity should be stated. From Table 1, it appears that the high BMI group is relatively low compared to most other populations. This issue should be discussed as a possible limitation of this study in the Discussion section. Composition of HFD should be disclosed either in terms of source of fat used or exact fatty acid and sterol composition.

4. Table 1: Table should also breakdown data and add separate categories for high TG versus normal TG in obese groups. Also not clear anywhere in text of figure legends the sample size for the high TG obese group. Results from Table 1 should be more thoroughly described in the result section. The data showing no difference in IL-10 mRNA in the total obese and nob-obese groups should be shown in the supplement or data added a sentence in the result section.
5. Rat study: Change in total cholesterol over time should also be shown. From the immunochemical staining, can the authors identify the cell source of IL-10 in the adipose tissue?

6. Other results: Why wasn’t serum IL-1- and RT-PCR results for STAT3 and ATGL not done on human samples? Addition of this data would significantly strengthen the paper.

7. Discussion: Authors should cite prior work by L. Freeman et al. in JCL on IL-10 and human lipid levels. One of the primary determinants of plasma TG is LPL and the various proteins (apoC-II, apoC-III, ANGTP3,4,8) that regulate LPL. This point should at least be mentioned in the Discussion section, but including some of these parameters at least the mRNA levels would considerably add to the paper.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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Please indicate the quality of language in the manuscript:

Needs some language corrections before being published
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