Reviewer's report

**Title:** Short-term outcomes of community-based adolescent weight management: The Loozit(R) Study

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**Reviewer:** Kerri K Boutelle

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Short-term outcomes of community-based adolescent weight management: The Loozit® Study

This manuscript presents pre-post data on 151 adolescents enrolled in a once a week group intervention. Adolescents and their parents attended 7 weekly group sessions, and anthropometry, blood pressure, and fasted blood sample were assessed pre-post treatment. Pre-post analyses suggested that there was a statistically significant reduction BMI and BMI z-score, waist circumference, total cholesterol and LDL. Authors also report improvements in dietary intake measures, physical activity, and sedentary behavior. Authors suggest that a greater availability of community-based adolescent weight management options is needed.

Overall, this is an interesting study, with relatively low drop-out rates, which contributes to the lack of data on community interventions with overweight teens.

Major Compulsory Revisions

My main concerns with this manuscript are language used in interpreting results. This study does not include a control group, and the pre-post data is used to evaluate changes in a cohort of participants after a short period of time, with no follow-up. The changes seen in weight and BMI and BMI-Z are relatively small. If you do the math, the participants lost on average, .2 kg or approx. half a pound. Although this shows a stabilization of weight in kg/lbs for 2 months, it is certainly not a panacea. The authors make broad statements suggesting that these programs need to be made more available to communities. I would strongly suggest that the authors temper their language related to these results.

Specific comments regarding the manuscript are listed below

1) What is the rationale for including only youth who are 1.0-2.5 BMI-Z? Why were youth above a BMI-Z score of 2.5 excluded? The rationale should be included in the manuscript.

2) How was secondary cause for obesity determined? Were participants screened for diabetes? Or was it self-report.

3) More information is needed on the intervention itself.

4) The authors should write out the percentage of the sample that is female,
instead of using an F.

5) The authors write that “In study completers at two months, 59% had reduced BMI, 73% had reduced BMI z-score, and 61% had reduced WC.” These statistics might be a bit misleading because participants who were included may have had a .001 reduction in BMI and included in this sentence. I would recommend the authors reporting on a clinically significant change, such as 5% reduction of body weight.

6) Why were there different levels of missing data for anthropometry and blood draw data? Please outline these in the paper to be transparent.

7) Authors use intent to treat to manage missing data, however, there are now more sophisticated methods to address missing data, such as multiple imputation. In the weight data, since there are so few missing, ITT is probably appropriate. However, 1/3 of the data is missing in the blood data.

Minor essential revisions

1) There is a line of research by Janicke and colleagues that describes a community intervention for younger obese children which has been relatively successful. I was surprised it was not included in the discussion of this paper.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests