Reviewer's report

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

Version: 1 Date: 3 June 2010

Reviewer: Susan Yanovski

Reviewer's report:

This is a report of the first 8 weeks of a large multisite clinical trial of adolescent weight management. Strengths include the obvious careful design of the study and rigorous measurement of outcomes such as anthropometrics. Limitations include lack of a control group or comparison between interventions and the short-term outcomes follow-up.

Major Compulsory Revisions

1) Page 9: It is good that you included ITT as well as completers analysis (although it is not always clear which analysis is used when reporting results—for example in the abstract). However, rather than replacing missing data with median value for study completers, a more conservative analysis would be to analyze dropouts using baseline observation carried forward, since those who dropped out were more likely to have return to baseline values.. Also-given the multiple anthropometric and metabolic variables being evaluated, did you adjust for multiple comparisons?

2) Page 12—I do not believe that you can call the BMI reduction you saw in the first 2 months of your program, which was small “clinically relevant” given that you have no follow up and no control group. As others are noting, there are secular trends toward decreasing or stabilizing BMI in children and adolescents in some populations.

Minor Essential Revisions

1): Although this study is cited as a “community-based adolescent weight management study” the intervention takes place in medical setting (community health centers), which many would not consider a community-based program, as these are often in non-medical settings—at least in the US. It appears that what the authors mean is that this intervention is not carried out in tertiary care/specialized treatment setting. It might be more descriptive to call this a primary care based weight management program—or at least use that descriptor early on in the text.

2) Page 2: Abstract-Results-you give percent male at follow-up, but not at baseline. Please note whether results were for completers or ITT.

3)Page 4: Study design: You need to give more of a description of the Loozit program—you cannot assume that the reader will take the time to read the cited descriptions—at the least, you should you should describe the length of the study,
the two arms used after the initial 8 weeks, and length and type of follow up. For example, you note in your flow diagram the children allocated to the two types of intervention but these are nowhere described in this paper.

4) Participant recruitment: According to the flow diagram, 68% of participants who were assessed were not eligible. A great majority of the were excluded for “not meeting inclusion criteria.” In order to determine generalizability of this study, it is important for you to give more information on these exclusions.

5) Page 6: Lifestyle behaviors-you note that “participants whose sedentary leisure activity time exceeded 72 hours/week were excluded according to established protocols.” Was this exclusion prior to randomization? If so, it should be noted in the exclusion criteria given in the section on study design. How many children were excluded for this reason?

6) Page 10: It is surprising that English was the primary language spoken at home by only 68% of the adolescents and that many parents were foreign-born. Is this typical of Australia? Were all subjects Caucasian? More information about demographics should be included-preferably in a table—including not only mean/median and SD but also range. Also-it would be useful to know if there are any difference in outcome by race/ethnicity.

7) Behavioral measures: All of your behavioral measures of dietary intake and physical activity are self-reported, which could be expected to change even in the absence of behavioral change, just by virtue of being in an intervention. Thus, I advise using the terminology “reported” meeting recommendations for diet, PA, etc. throughout. This limitation should also be addressed in the discussion.

8) Page 12: Discussion—many people would not consider an intervention carried out in health-care setting to be a “community-based” intervention, but rather a primary care intervention. Also— in first paragraph improvements in lifestyle behaviors should be noted as “self-reported improvements” since there are no objective measures.

9) In paragraph 2, you note that published studies of low to moderate intensity group interventions “were published over twenty years ago”—there are a number of recent studies carried out among adolescents in primary care settings (for example, Saelens et al. Obesity Research (2002) 10, 22–32; . I suggest conducting an additional Medline search to include some of this literature. In addition, you note that these “were at least double the duration of the Loozit Program” – however, the results you are presenting are only for 8 weeks of a longer intervention; thus, they should not be presented as stand-alone data from an 8 week program. In addition, you have no long-term follow-up.

10) Page 11 bottom—you know that interventions having “greater intensity of contact” had at least bi-weekly contact for 3-6 months” – again, you had weekly contact for 8 weeks, but not sure you can compare these studies and call q 2 week (bi-weekly) intervention greater intensity, since you are only presenting the first 2 months of an ongoing program.

Discretionary revisions
1) Lack of page numbering or line numbering makes providing comments
difficult-I have self-numbered the pages starting with 1 for the title page.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.