Reviewer's report

Title: Effects of a population-based overweight-prevention intervention on health behaviors of 5 to 7 year old children: the 'Be active, eat right' study

Version: 2 Date: 6 July 2013

Reviewer: Susan Sisson

Reviewer's report:

Thank you for the time invested in this manuscript preparation. The purpose of this study was to examine the obesogenic behaviors and possible change with intervention in overweight children and their families in The Netherlands. Although the lack of significant findings is a bit disappointing given the rigor in which this study was conducted, these findings should be presented. However, the greatest weakness of this paper is they lack of explanation of fidelity of the intervention as well as not actually testing families on the constructs which they received in the intervention. This must be addressed. I’ve provided comments detailed below. However, it does seem a bit long and with a very large amount of references and lengthy tables.

Abstract:

In the objective, I think the phrase about the origin of the study data (Be active, eat right study) is confusing. It causes the sentence to not flow really well. A better place for that information is the methods.

The objective should state, however, that it was in overweight children. This means it is not really overweight prevention, but weight reduction or obesity prevention. But if they were already overweight it can’t really be prevention.

The BMI data should be presented as either BMI percentiles or z-scores, whichever the authors feel is appropriate. Raw BMI should not be used in pediatric populations.

Results could be presented in a way that is clearer with significant results presented first followed by non significant ones. As written it is a little confusing. Or perhaps present the actual data for the findings that are significant and not the odds ratios and parameter estimates for those that are not significant.

Introduction:

In the 3rd paragraph the authors identify it is an obesity prevention interventions not overweight prevention intervention. Please correct the abstract.

First sentence, last paragraph. It is a bit confusing… The authors state a subsequent paper will have to be published to address the obesity outcomes, but they provide a reference that is 4 years old. Is that not the obesity outcomes/effectiveness? If so, it would not really be subsequent to a paper that is not yet even in press. Please clarify and/or rephrase.

Intervention:
The content of this first sentence has been repeated three times including in the introduction and the methods prior to this intervention section. I don’t think this level of repetition is necessary. Please modify.

As the methods state all these intervention details have been previously published, it seems a bit lengthy to go into these details. As the overall paper is rather long, why not simply refer the readers to the original intervention description and avoid repeating? At minimum this section should be shortened to quickly focus on the relevant items.

Data collection: While the authors state the variables were dichotomized, how were they ascertained on the survey? Was it continuous variables the authors categorized or categorical that were collapsed?

On the vegetable servings, how big of a spoon? Please provide more clarification. If this is just a regular eating spoon the servings of vegetables could be much higher than for fruit since several eating spoons (~ tsp) would equal one medium apple or banana. Were parents provided any visuals to assist or given any instruction at the intervention or well child visits to help them gauge an appropriate serving of vegetables?

Parenting practices:
How can the range of healthy (1-8) overlap with the range of unhealthy (1-4)? Is this a typo? If not, please provide appropriate data clarification.

Results:
Section on sample characteristics… Please say “was male” or “were boys” “was boy” does not read appropriately in English.

Section on child health behavior outcomes… Please add an “ly” to “significant higher percentage”. Again, English grammar appropriateness.

Am I reading the rest of this sentence correctly? After the intervention, the kids spent less time outside and more time watching TV than at baseline?

Given the lack of significant findings, more attention should be given to the adherence to the protocol and intervention. If those data are presented in another publication, it should be referenced and clearly acknowledged.

Discussion:
Needs to be more focused and succinct.

Given the lack of significant findings from this intervention. Space needs to be dedicated to speculating why and providing suggestions and/or lessons learned for future interventions. While non significant findings can be disappointing, they can tell us a lot about what went wrong and what should be done differently. This should really be expanded for this paper to contribute to the literature.

It could be due to poor compliance with attendance and high attrition. Although intent-to-treat analyses were appropriately used by the authors.

First paragraph, the authors note a “significant positive” change. Please rephrase to “healthful”. The verbiage positive can be confusing as a positive change might be misconstrued for an increase. And an increase in sugar-sweetened beverages
would not be healthful. This will help convey your meaning more adequately.

Why would you even expect a change in body composition/BMI if there was no effect on any of the behaviors? I would recommend removing that from the paper completely (intro and discussion) and focusing on the behaviors and intervention at hand.

If 1-2 behaviors were discussed with a patient, were those noted? Participants should have only been evaluated on those elements that were actually in their personalized intervention. This should be explained and clarified. That may explain a lack of findings, participants were being evaluated on metrics which they should not have been expected to change and were therefore not really in the intervention. Even though a lack of power is cited as why this was not done, it should be explored given the lack of other outcomes as a group.

A stated strength was the broad acceptance of the program – with only ~30% attending 3 session of intervention, I would not call that broadly accepted. Perhaps you can rephrase to focus on the broad implementation of physicians or something, but actually few families accepted the intervention.

A clear limitation seems to be the intervention fidelity monitoring and compliance by physicians and attendance of families. Yet, nothing is mentioned about this glaring issue in the limitations. Please add this into the discussion. Additionally, not actually measuring families on the content they received is a major limitation of the current analyses. Should the author redo that component, it can be strengthened.

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

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

no competing interests