Reviewer's report

Title: Solution-focused low-intensity intervention in improving health behaviors of young females: cluster-randomized controlled trial

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Reviewer: Leif Edvard Aaroe

Reviewer's report:


This study has several strengths: The intervention under focus in this study is highly relevant for public health and it rests on a well described rationale. The external validity of the study is high. The number of observations is large. Outcomes of the intervention are analysed with an intention to treat approach (ITT) as well as the per-protocol technique (PP). The article is relatively well written. The constellation of findings in this study (insignificant change for three health behaviours separately, but an overall significance) requires a high level of precision when describing and discussing the findings. It is my impression that the authors have succeeded well also on this point.

As almost always, however, there are some issues that need to be dealt with before the paper is sufficiently strong to be published in BMC Public Health.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

1. This is a cluster-randomized trial. I am not sure what the clustering variable is. Did you randomize with the 15 study site communities as the cluster variable? This sentence (page 6) indicates that you did: “The participants were cluster-randomized into the intervention and control groups based on the vaccination study site (community) …” How many clusters did you have in each group? This needs to be mentioned.

2. As far as I understand from reading the methods description no adjustments for cluster effects have been carried out. Usually such adjustments contribute to increasing standard errors of estimates. Confidence intervals get larger and it becomes more difficult to obtain significant results. This issue must be dealt with. One possibility is to use the Complex module in SPSS, the corresponding modules in STATA or specialized software like Mplus. There is no need to use multilevel modelling. In SPSS it may be difficult to find cluster-adjusted techniques equivalent to some of the non-parametric techniques you have used in this article. You may have to manage with variants of GLM, logistic regression and analyses of crosstabs. If you only have randomized 15 clusters (with perhaps 7 in one group and 8 in the other), it is extremely difficult to obtain significance. A large number of subjects does not compensate for a low number
of clusters. You should still try and see what you get. If you have randomized as few as 15 clusters into two groups, it should be mentioned and discussed as a serious limitation of this study.

3. On page 17 you say that “Despite the cluster-randomization, there were some baseline differences in the level of physical activity, BMI, and the current educational level between the intervention and control groups.” I believe you have got significant differences because of the cluster randomization and not “despite”. If you test the differences with adjustments for the cluster effect, I guess the significances will disappear.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

4. On page 4 (second last paragraph) you say that “In Finland, promoting the availability and usage of healthy nutrition and physical activities is common …” Does it make sense to talk about the availability and use of physical activity? To me this sounds like a strange expression.

5. The intervention is called LINDA and this study is sometimes called the LINDA study. It would be great to know what LINDA stands for. Is it an abbreviation?

6. Under “Description of the LINDA intervention” (page 6) there is one paragraph that sounds like this: “The main outcomes measured were changes in the physical activity level, meal regularity, bedtime before school or workdays, and Body Mass Index (BMI). In addition to comparing all participants in the intervention arm to the controls (intention-to-treat analysis), we compared to the controls only those participants in the intervention arm who chose to discuss their behaviors with the study nurses, based on the study records of the health nurse, and thus actually received the intervention counseling (per-protocol analysis)”. This is not about the LINDA intervention and may already be sufficiently well described under “Statistical analyses”. Please check.

7. Would it be possible to say more about the measures used in this study? Were he questions and scales adapted from previous studies? Could you provide some information about psychometric properties, first of all reliability and validity?

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

8. Would it change your results in any way if you analysed BMI as a metric variable instead of using it as a categorical variable?

And finally my responses to some issues raised by BMC Public Health

1. Is the question posed by the authors well defined?
   Yes, crisp and clear.

2. Are the methods appropriate and well described?
   Yes, except that we need to know more about the quality of the instruments used during data collection.
3. Are the data sound?
Yes. That is my impression.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
Yes, that is my impression. In this kind of studies there is usually a tendency to draw positive conclusions based on rather weak findings. In this study there are few significant intervention effects. I have re-read the paragraphs where the results are summarized, and it is my impression that they have chosen phrases that are just on the right side of this thin line between being honest to the data and marketing their findings. In light of the new analyses that they have to do (adjusting for the cluster effect) they may need to change their discussion and conclusion, but before they have done these analyses, we don’t know how.

6. Are limitations of the work clearly stated?
Clustering a limited number of units appears to me as a serious shortcoming that is not at all discussed. This is dealt with in my comments above.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
I believe they do.

8. Do the title and abstract accurately convey what has been found?
Yes that is my impression, although this may have to be changed a bit after controlling for cluster effects.

9. Is the writing acceptable?
I believe the language is fine. My native language is, however, not English. I recommend that the language of the manuscript is examined more closely.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**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