Reviewer’s report

Title: The Program SI! intervention for enhancing a healthy lifestyle in preschoolers: First results from a cluster randomized trial

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Reviewer: Freia De Bock

Reviewer’s report:

This paper reports on the effectiveness of a year-long intervention in 12 preschools in Spain. Effects of the intervention on 3-5 year-old children’s outcomes such as attitudes and health behaviors are compared to changes observed in children of 12 control schools. The design is a sound cluster-randomized trial and the statistical analysis using a mixed linear model and testing interactions seems reasonable.

The intervention approach is innovative as it targets -in addition to traditional lifestyle factors such as diet and physical activity- the children’s emotional control (as part of an integrative approach). The paper however does not give detailed explanation of what really was done in the intervention schools to promote emotional control of children, nor can this information be found in the cited study design paper. Also, details on which items are contained in the outcome scores are lacking, both in the manuscript and in the referenced BMC Public Health Paper.

The involvement of parents in the SI! intervention relies on health education strategies, e.g the provision of a website and homework tasks over the weekend. Previous research however has repeatedly suggested that health education and knowledge communication might not be sufficient to achieve behavior change – which might be mirrored in lacking effects on the parent and teacher level in this study.

Another explanation for lacking effects on the parent and teacher level could be that the intervention did not reach into these target groups. However, the authors do not provide any information on the issue of to what extent the target groups were reached. Modern articles on intervention effectiveness should at least incorporate some measures of intervention process, e.g. by describing reach, adoption, fidelity, maintenance of the intervention. The authors do not take this opportunity, probably because they do not have data on the intervention process. This however is a major shortcoming of the manuscript.

- Major Compulsory Revisions

1. The authors’ comparison with previous literature is limited to a study in Colombia (which is their own) and one in native Americans. The authors should cite other relevant studies e.g. in UK and Europe (countries which might be more
comparable to Spain) and compare with intervention studies in preschool age, which used behavioral outcomes such as diet or physical activity as outcome measures.

2. The clinical relevance of the changes in the “knowledge-attitudes-habits” scores remains unclear. What does a change of 3.45 on a scale from 0-30 mean in terms of health benefits? The authors have to discuss the potential health benefits of the improvements on their scores.

3. This underlines a general problem of this study: Interpretation of effect sizes in the unit of scores, which are not commonly used and under-explained in the paper seems hardly possible for readers. The referenced BMC Public Health Paper does not describe the specific items used for the author’s outcome scores and their measurement, either. The authors should provide more information for clarification. They should also explain which characteristics of the school environment were measured. It is not enough to mention that the teachers filled out a questionnaire (page 7).

4. More information is needed on the specific intervention strategies to promote emotional wellbeing, as they seem to be the innovative part of the SI! Intervention compared to previous dietary and physical activity interventions in preschool age.

5. There are several limitations that the authors should mention:

6. A limitation of the current study – in comparison with previous studies – is the lack of any “hard outcomes” such as anthropometric. In most previous intervention studies, short-term behavioral changes as reported in this study have been shown to be not sustainable, so the overall intervention benefit is unclear unless anthropometry outcomes are included. The authors have to discuss this issue.

7. The authors use the „social cognitive model“ and trans-theoretical model as theoretical framework for their intervention approach and their outcome measurements in young children. However, several authors have noted that differences between dietary behaviours and the addictive behaviours upon which the transtheoretical model was originally based may cause problems for the model (Povey et al, Health Educ. Res. (1999) 14 (5): 641-651.). Also, a transfer of the stages of change model to children of young ages seems questionable (Corden and Somerton, British Journal of Social Work (2004)34,1025–1044). The authors should – if possible - reference papers investigating the validity and the transferability of both the social cognitive model and the stages of change model to children of ages 3-6 years. The sentence „This structure corresponds to children’s cognitive development from understanding concepts (K), through intending to set this knowledge into practice (A), to eventually acquiring the desired behavioral pattern (H).“ should be formulated much more cautious (or a reference should be given). The potential invalidity of theory in young preschool children should be mentioned in limitation section.

8. The manuscript lacks details on the intervention’s processes: If no effects in
teachers nor parents were seen, is this due to a low reach into the teacher population? How about the fidelity with which the intervention was delivered? Was it maintained throughout the school year? The authors should present at least some intervention process data, otherwise the results and the relevance of the effects are not well interpretable.

9. The involvement of parents in the SI! intervention relies on health education strategies, e.g the provision of a website and homework tasks over the weekend. Previous research has shown that health education and knowledge communication might not be enough to achieve behavior change – which might be mirrored in lacking effects on parent and teacher level in this study. Please discuss this and mention under limitations.

- Minor Essential Revisions
1. Page 4: Please mention the primary outcome on which you powered the study. Also explain, to what extent your study was powered for the subgroup analyses presented in figure 2.

2. Page 5: the exclusion criteria mentioned are too vague: what was a high percentage of immigrant background?

3. Page 7: Please include the intervention website’s url-address to check for the interested reader.

4. Page 12: The authors’ comparison of their own behavioral outcomes with studies using objective anthropometric outcomes is inadequate. The authors do not know whether their intervention is more successful than “Previous school-based intervention studies that focused on clinical outcomes” as they have not measured clinical outcomes. Please rephrase.

- Discretionary Revisions
1. page 13: “would be necessary” -Please add dot at the end of the sentence.

**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:**

I declare that I have no competing interests