Reviewer’s report

Title: A family-based education programme for obesity: a three-year study.

Version: 8 Date: 9 July 2007

Reviewer: caroline braet

Reviewer’s report:

The authors have been quite responsive to the previous comments. The restructuring of the introduction and methods-sections seems to be helpful for the reader. Also, several issues regarding the analyses and the results were addressed. Finally, the discussion was changed according to the suggestions.

We still have three unsolved important comments and some new issues:

Comments:

1. More information on the assessment phase should be included, describing the measurements used (especially because on p. 9 is written that children with body-image dissatisfaction, emotional eating and binge-eating received additional sessions – how was this measured?)

   The authors argue that the paediatrician evaluated the possible occurrence of body-image dissatisfaction, emotional eating and binge eating on the base of a clinical evaluation and her experience. However, it is generally accepted that diagnoses of eating disorders should be based on standardized clinical interviews or at least on well-validated questionnaires. We suggest that this should be addressed as a limitation in the discussion section.

2. The description of the study population (p. 5 – 7) raises several issues of concern: matching of the children, description of the sample with regard to their ethnicity, socioeconomic status and other related demographic characteristics should be presented in the text.

   Acknowledging this lack of information in the discussion section seems reasonable.

   Furthermore, we still question the matching of the children, no matching rules were used (see page 7 ‘an uninfluenced choice‘ (by the families) (based on what??))
3. Programs. What are similarities and differences between TEP and DT? This is especially important for the appraisal of potential overlap between both conditions. Are both condition checked on treatment integrity? Do the therapists had contact with each other? How to avoid contamination of the treatment approach?

We suggest that the authors should address this issue in the discussion section.

New comments:

1. On page 4 of the introduction, the authors write that Braet and Golley reported a drop-out percentage of 20%, while there was referred to Quattrin and Golley. Please check the references.
2. On page 4 of the introduction, 5 to 10% weight loss is defined as a good therapeutic result. Please give a reference.
3. In the part on the Participants:
   a. What is meant by: (range 1-5.6 years, mean 2.7)?
   b. Please provide data on parental BMI for both groups.
4. Could baseline difference in BMI% in both groups have influenced the outcomes? Please address this in the limitation section after checking if the difference is significant (see page 6).
5. The text needs language adaptations. Please contact a native English translator.
   • Eg page 3 ‘traditional dietary treatment the reduction was 42.8%’ should be reformulated as ‘in traditional dietary treatment 42.8% of the children succeeded in BMI% reduction’
   • Eg page 3, first paragraph ‘Weight reduction was good in moderately obese children as well as in severely ones. In addition, compared to dietary approach, a smaller percentage of the children of the study had a BMI increase of >10%. Finally, drop-out rate differs between both interventions and, using follow-up phone calls, the drop-out rate fell to 2%.
   • Eg page 4, second paragraph. The authors use ‘it tends’ and ‘it is inversely related’. Please replace ‘it’ by ‘drop-out rate’.
   • Eg page 4, fourth paragraph. ‘The Israel Consensus Statement established that any z-BMI’ add: ‘change or decrease’ and delete the second
‘that’.

• Eg page 13, second paragraph. Please verify in depth the use of ‘increase’ and ‘decrease’ in this section.