Author's response to reviews

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

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Author's response to reviews:

To Dr Annabel Phillips
Senior- Assistant Editor, BMC-series journals
Ferrara 28 September 2007

Dear Editor,

thank you for the careful revision of our manuscript: "A family-based education program for obesity: a three-year study", to be considered for publication as a Research article in "BMC Pediatrics".

We changed the text according to the reviewers' suggestions. We also provided a letter with a point-by-point answer to the reviewers' comments. We made the required changes to the manuscript's format and entered the references according to the standard style of your Journal (Microsoft Word Template). We also provided figures as separate files, after trimming them to optimise the space. The manuscript has been reviewed by a professional English translator.

Yours sincerely,
on behalf of all authors

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A family-based education program for obesity: a three-year study.
Rita Tanas, Renzo Marcolongo, Stefania Pedretti and Giuseppe Gilli

Comment 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.

In the discussion section (page 16, paragraph 6, lines 5-6) we state that this is one of the limitations of our study. We mention that the evaluation of concurrent psychological problems body-image dissatisfaction, emotional eating and eating disorders was performed by the pediatrician without a standardized clinical interview and/or some well-validated questionnaires, as is usually done.

Comment 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.

Page 6, paragraph 3, last line states that the study population included only Caucasians. In the Suggestions and Conclusions section we mention that the lack of socio-economic and other cultural/demographic evaluation in the 2 groups is a limitation of this study. On page 19, paragraph 1, lines 6-8 we state: it would be important to better understand the influence of family social and economical conditions and of the degree of obesity (initial BMI%) on the final results of our study.

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??)

Concerning the selection of the 2 groups of subjects, we have added the following explanation in the Participants section (page 7, paragraph 2):

Case allocation between the two groups was completely fortuitous; indeed, when families called our center asking for advice and consultation, they made a completely free and uncoerced choice between different healthcare professionals who work in two separate clinics at the same unit and to whom the public has access in a random fashion based on availability.

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.

In the discussion section (page 18, paragraph 1-2) we have added,

Moreover, it should be underlined that, compared to DT, TPE does not involve any prescription, but simply trusts the children and their families, promotes their self esteem and puts them in the condition of making free and responsible choices. By contrast, DT adopts a dietician-centered, prescriptive approach, that tends to substitute parental role and to reduce parents' self esteem, rather than helping them to develop a self-determined and responsible attitude toward their child's health.

During the study period no information had been exchanged between the two therapists.

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.

We apologize for the error and have corrected the reference number

2. On page 4 of the introduction, 5 to 10% weight loss is defined as a good therapeutic result. Please give a reference.

Two references have been provided

3. In the part on the Participants:
   a. What is meant by: (range 1-5.6 years, mean 2.7)?

For some reason a line of text was moved from its original position rendering the sentence unclear. This has been corrected.

b. Please provide data on parental BMI for both groups.

The data concerning the BMI of parents has been added to the Methods, Participants section.

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).

The difference is significant as explained in the Methods section, under Participants (page 6 paragraph 3, line 5). The mention of this limitation of our study has now also been added to the Suggestions and Comments section.(page 19. paragraph 1, line 6-8).
5. The text needs language adaptations. Please contact a native English translator.

The manuscript was reviewed by a professional 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¿

The suggestion has been accepted.

Eg page 3, first paragraph. 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%.

To clarify the sentence we have modified the text as follows: In addition, a smaller proportion of children treated with therapeutic education had negative results (BMI increase of >10%) compared to those treated with dietary approach (11.8% vs. 25.7%);

Eg page 4, second paragraph. The authors use ¿it tends¿ and ¿it is inversely related¿. Please replace ¿it¿ by ¿drop-out rate¿.

This correction has been made.

Eg page 4, fourth paragraph. ¿The Israel Consensus Statement established that any z-BMI¿ add: ¿change or decrease¿ and delete the second ¿that¿.

The word ¿decrease¿ has been added. We apologize for this oversight. The second ¿that¿ has been deleted.

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

We have verified and corrected the use of ¿increase¿ and ¿decrease¿ in this section. We apologize for the error.

Reviewer’s report
Title: A family-based education program for obesity: a three-year study.
Version: 8 Date: 6 July 2007
Reviewer: Sandra Plachta-Danielzik

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct) The coeval use of BMI, %BMI, z-BMI and BMI-SDS is very unusual and causes confusion. In general, BMI-SDS is used instead of z-BMI when the distribution of BMI is not normal contributed
(which is the case in children). I think that the results differ in this extent because the authors did not use the same reference population to calculate z-BMI and BMI-SDS.

We have re-calculated the BMI-SDS for each child before and after treatment by adapting the precise age with Cole¿s formula with the data in the prepared tables, relative to the same reference population of Luciano. As you foresaw, they are nearly identical, so we have substituted z-scores with BMI-SDS values, better suited since the distribution of BMI in not normal, as you suggested.

Discretionary Revisions (which the author can choose to ignore)
What next?: Accept after minor essential revisions
Level of interest: An article of importance in its field
Quality of written English: Needs some language corrections before being published

As mentioned above, the manuscript has been reviewed by a professional English translator.