Author's response to reviews

Title: The impact of overweight and obesity on health-related quality of life in childhood - results from an intervention study

Authors:

Nora Wille (n.wille@uke.uni-hamburg.de)
Michael Erhart (m.erhart@uke.uni-hamburg.de)
Christiane Petersen (christiane.petersen@mobydickhamburg.de)
Ulrike Ravens-Sieberer (u.ravens-sieberer@uke.uni-hamburg.de)

Version: 3 Date: 10 December 2008

Author's response to reviews: see over
Dear Dr. Norton,

Thank you very much for your response from November 27th regarding our research article “The impact of overweight and obesity on health-related quality of life in childhood – results from an intervention study”. We were delighted that you in principle accepted our paper for publication.

In compliance with your recommendation the manuscript was copyedited by native English speakers in the meantime. The English language was revised by “American Journal Experts” and should be suitable for publication now. Furthermore we ensured that the manuscript conforms to the journal style.

We are looking forward to your reply.

Sincerely,

Nora Wille
Reviewer 1

Reviewer's report

Title: The impact of overweight and obesity on health-related quality of life in childhood - results from an intervention study

Version: 1 Date: 26 September 2008

Reviewer: Juliana Kain

RE: Thank you very much for the review, which we found very constructive.

Reviewer's report:
I consider that the results of the cross-sectional analyses are valuable, while the longitudinal one has several limitations, which are pointed out by the authors and I will mention them again:

Reviewer 1; comment 1:
- There is no mention about the overall socio-cultural characteristics of the children in both groups; are they comparable?

RE: In our revision information on socio-cultural characteristics was included in the sample descriptions (see below). Since available information on perceived family wealth (“How well off do you think your family is?”) and migration background indicates comparability of the groups, there are no reasons to assume that the groups differ on important socio-cultural characteristics.

Additionally included in the manuscript now:
“Of this sample, 93% of the children were born in Germany; however, 39% reported that languages other than German were spoken at home. Sixty percent of the children rated the financial circumstances of their family as good, 33% as average, and 7% as poor.”
“Of these children, 92% were born in Germany, but 38% reported that not only German was spoken at home. Fifty-eight percent of the children rated the financial circumstances of their family as good, 30% as average, and 12% as poor.”

Reviewer 1; comment 2:
- The authors report that there is no data on the intensity of treatment, how many sessions did the children attend and what activities were carried out. The information on treatment is minimal and it has to do with one of the objectives of the study

RE: We agree with this remark and added as much detail as possible in order to better describe the treatment and participation. However, even though it was desirable to insert even more exact details, the revised description also reflects the common practice in such outpatient programs. These are often standardized only to a certain degree and, therefore, difficult to describe exactly. This disadvantage in internal validity may be counterweighed by higher external validity. The outpatient program was conducted as it is done in everyday life and did not follow stricter rules of implementation than usual due to ongoing evaluative research.
Additionally included in the manuscript now:

“The treatment included participation in the one-year German outpatient program “Moby Dick”. The program consists of attendance in a group that supports a balanced diet, regular exercise, as well as the psychosocial well-being of the participants and aims to stabilize or reduce the participants’ weight in the long-term. The children visited the group one afternoon per week for three hours. During these afternoons, the children first took part in a sport program for 1.5 hours that aimed to create playful experiences of success by means of joining safe physical activity games with special attention to injury prophylaxis. Afterwards, the children received lessons on nutrition, cooked together, and participated in role playing and other pedagogic interventions to enhance self-esteem and appropriate problem-coping behaviors. During all school holidays, a program covering between three and five days a week was offered with one compulsory date each week. The holiday programs aim to promote active leisure time behavior and to support new peer relationships through excursions, workshops, games, and sport competitions. The children register to participate in the different activities, which are partly carried out by external cooperating partners such as sport clubs or youth clubs. In addition to the child-centered part of the program that included at least 52 weekly appointments during one year, the parents and further significant others (e.g., grandparents, guardians) participated once a month in a meeting and received training in order to support the progress of their children. The sessions covered topics such as nutrition, medicine, physical activity, strengthening personality, and solving conflicts. Six meetings in the one-year program were parent-child afternoons.”

“However, the groups are characterized by a high commitment. If a child could not attend a group meeting, the parent was required to excuse the child beforehand. Since parents only receive reimbursement for the program fees from their health insurance in the case of regular attendance (80% of appointments), there is also a financial incentive to attend the group regularly.”

Reviewer 1; comment 3:
- As the authors point out a limitation is the use of self-reported weight and height. Did a authors check this data with actual measurement?

RE: We did not have the opportunity to directly measure weight and height. In order to clarify this issue, we inserted this information (in italic) in the following sentence in the discussion:

“However, the limitations of the study should be considered. Due to the self-report of height and weight, which we relied upon without collecting actual measurement, the reliability of the BMI data is questionable [31].”

Reviewer 1; comment 4:
- In the statistical analysis, the first line says ”to assess differences” ...in what?

RE: The term “in subjective health” was added in the text to clarify the meaning of the sentence.

Reviewer 1; comment 5:
- In the longitudinal analysis, the results are questionnable due to the limited Power

RE: As the review pointed out, we elaborated on this issue in the discussion. We think that two aspects should be taken into account regarding this matter.
First, the actual sample size of the longitudinal analysis (n=80) enables us to detect middle-sized effects (d=0.5) with a satisfactorily power of p=0.99. The power is only limited regarding the detection of small effects: an effect of d=0.3 is detected with a power of p=0.76. However, this is not strongly below the critical value of p=0.8. Nevertheless, we agree that this is obviously a limitation. Therefore, second, we think that it is important to notice that limited power only affects the interpretation of results when no differences or associations are observed. However, we found changes in HRQoL in the longitudinal analysis despite limited power and, thus, regard these results as valid and interesting. In order to additionally clarify that the lack of significant differences on other scales does not justify the conclusion that there are no improvements on these scales, we added an additional sentence pointing out that some statistical tendencies were found and might hint at further pre-post differences that were not detected due to limited power.

Additionally included in the manuscript now:
“The statistical tendencies of physical and psychological well-being as well as self-esteem hint at possible further pre-post improvements that might have been undetected due to limited power.”

Reviewer 1; comment 6:
- Because no information is known about the treatment, no conclusions can be drawn from it.

RE: As we pointed out above, we inserted more details regarding the treatment program and with respect to participation of the children.

Reviewer 1; comment 7 (Specific Comments):
Abstract: Background is too long. In Methods, include some statistical methods used

RE: Background in the abstract was shortened from 66 to 51 words. Statistical methods were included in the abstract’s methods section.

Reviewer 1; comment 8 (Specific Comments):
Figure 1: in T2 replace one of the blue colours for another colour.

RE: The blue color was replaced by green. Furthermore, the background of the figure was turned from grey to white in order to make recognition of the figure easier.

Reviewer 1; comment 9 (Specific Comments):
The article needs some English editing.

RE: After revision, the article was edited by a professional service (American Journals Experts).

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being Published
RE: As pointed out above, the article was edited by a professional service (American Journals Experts) after revision.

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Reviewer 2

Reviewer's report

Title: The impact of overweight and obesity on health-related quality of life in childhood - results from an intervention study

Version: 1 Date: 7 October 2008

Reviewer: Ludwig Fahrmeir

RE: Thank you very much for the review, which we found very constructive.

Reviewer's report:

Major Compulsory Revision:
Statistical analysis relies on classical two-samples and paired samples tests as well as correlation coefficients to test hypotheses relevant for the substantive research questions of the paper. The basic assumptions of these tests are that the samples are i.i.d., that is (more or less) homogeneous with respect to age and sex (and possibly other confounders). On page 3, at the beginning of the section Statistical Analysis, the authors state that they controlled for age and sex through an analysis of variance, without showing results. I would like to see more details of this analysis of variance.

RE: More details regarding this analysis of variance were added in the text. The results were inserted in terms of F- and p-values.

Additionally included in the manuscript now:
“All of these results were confirmed by a univariate analysis of variance controlled for age and sex, which found significant differences exclusively with respect to the general health item (F=19.34; p<0.001), the KINDL® ‘friends’ subscale (F=7.27; p<0.007), and the ChildDynHA Scale (F=8.57; p<0.004).”

Discretionary Revision:
The sample sizes are comparable moderate so that the power/significance of statistical results is also moderate in some cases. I think it would therefore be reasonable to provide some additional explorative results, for example comparing box plots instead of only providing t- and p-values in Tables 2 and 3, or showing scatter plots for BMI scores versus quality of life scores. This could help to support (or to generate) hypotheses and, at the same time, show the empirical distribution of the data.
RE: We discussed this comment extensively and thought about possibilities to present further additional explorative results. However, we decided against it for several reasons:

(1) Associations between BMI and HRQoL must be based on BMI z-scores. However, as discussed in the article, reliability of zBMI is assumed to be limited due to self-reported data. This is the reason why we focused more on groups of BMI percentiles as well as categories of improved weight or not improved weight since these broader categories are less prone to bias. Scatter plots with zBMI as one variable would be misleading by implying higher reliability of BMI z-scores.

(2) The addition of box plots in order to present the data of Table 2 and 3 would present similar information twice. On the other hand, we feel that we cannot delete the tables since they include the most important information that can only partly be presented in graphics. We thus would like to ask the reviewer to retain the tables 2 and 3 and not to replace them by figures.

(3) As it was outlined above, we discussed the issue of low power and significance extensively in the discussion and added some further information during revision. With regard to this full description, we think that is not necessary to add further graphics.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.