Author’s response to reviews

Title: Predictors of overweight and obesity in five to seven year old children in Germany: results from cross-sectional studies

Authors:

Christian J Apfelbacher (christian.apfelbacher@web.de)
Adrian Loerbroks (adrian.loerbroks@psychologie.uni-heidelberg.de)
John Cairns (john.cairns@lshtm.ac.uk)
Heidrun Behrendt (heidrun.behrendt@lrz.tu-muenchen.de)
Johannes Ring (johannes.ring@lrz.tu-muenchen.de)
Ursula Kraemer (kraemeru@uni-duesseldorf.de)

Version: 2 Date: 27 March 2008

Author’s response to reviews: see over
Response to reviewer reports

Re: Reviewer reports regarding manuscript by Apfelbacher et al. “Predictors of overweight and obesity in five to seven year old children in Germany: results from cross-sectional studies”

Reviewer 1

Reviewer's report

Title: Predictors of overweight and obesity in five to seven year old children in Germany: results from cross-sectional studies

Version: 1 Date: 18 December 2007

Reviewer: Cristina Padez

Reviewer's report:

Major compulsory revisions

Overview: This paper reports overweight and obesity prevalence rates for children aged 5 to seven years in Germany, as well as some risk factors associated with overweight and obesity in the same population.

Opinion: This paper should be published in BMC Public Health after some changes. The revision is especially in which concerns some methodological problems, particularly definitions of variables. Introduction sections also need a better improvement.

The are, also, new works published in 2006 and even in 2007 about predictors of childhood obesity that the authors must mention. They must use other works beside the ones published about the Germany population.

Response:

We are grateful to the reviewer for pointing out that discussing our results in the light of international studies is important. We have therefore improved the discussion section, and now quote the review by Shrewsbury and Wardle which was just published last month on page 11.

The sentence: “A study performed in East German children found an inverse association between parental education and both overweight and obesity for university degree education only [4], while in our sample each increase in parental educational level was related to a reduced odds of overweight and obesity.“ has been deleted.

We are now also citing the study on overweight in Swiss children by Lasserre et al. on page 12, and the paper from the European Youth Heart Study by Ortega et al, the study on prevalence and risk factors for overweight and obesity in Portuguese children by Padez et al., the study on prevalence and determinants of obesity in children in public schools in Sintra/Portugal by Ferreira et al., and another German study from Kiel by Danielzik et al. (page 13).
A paragraph discussing the results of two meta-analyses about the role of breastfeeding (Harder et al; Owen et al.) has also been inserted in the manuscript (page 14). The paragraph on the role of birth weight has been expanded in the light of international literature (page 14).

**Recommended alterations:**

- Abstract number of males and females should be provided in the abstract as well as the prevalence of overweight and obesity by sex.
- The cut-off points used must be indicate in the abstract section.

Response:

The percentage of boys has been added to the methods section. Likewise, the prevalence of overweight and obesity has been added to the results section. The sentence “The cut-off values ranged from 17.15 kg/m² for overweight girls aged 5-<5.5 years to 21.09 kg/m² for boys between 7.5 and less than 8.0 years of age.” has been slightly changed and was inserted in the manuscript as “The cut-off values ranged from 17.2 kg/m² for overweight girls aged 5-<5.5 years to 21.1 kg/m² for boys aged 7.5-<8.0 years.” A detailed list of the cut off points that were used can be found in our previous article (Apfelbacher et al. 2008).

- Background: For children there are much more adverse health outcomes that should be describe as well as potential predictors of childhood obesity. The authors should describe better those factors and joint references. There are many that must be referred and not only in Germany population.

Response:

The paragraph on adverse health outcomes of childhood obesity in the background section has been expanded and references have been included. A number of studies on predictive factors are now being cited in the background section. The sentence “However, the findings for some potential predictors, for instance breastfeeding, are inconclusive.” has been deleted in the light of the now cited meta-analyses. “Those German studies” has been replaced by “The cited studies”.

- Methodology
  - Study design: the authors should, at least, mention that the methodology used (many anthropometric observers) could provide some errors in weight and height means.
  - Outcome variables: it is not necessary to describe the BMI values used. The last sentence should be deleted.
  - Parental education: were these intervals national or the authors constructed their own intervals. I believe Germany as National Intervals for education, if this is the case the authors must use the national ones. Is the father and/or mother education? Or both?
  - Breastfeeding intervals: why only this interval > 3 months? With a large sample the authors should use more intervals none, 1-3 mo, 3-6 mo and > 6 months.
- How was urban and rural defined? Again the same recommendation that was used for parental education. Is there a national classification? Or is this the authors classification and based in what criteria?

Response:

The reviewer rightly points out that the methodology used might have introduced measurement error in weight and height measurement. This is now discussed in the last paragraph of the discussion.
The sentence referring to BMI values was deleted and has been moved to the abstract sentence in a slightly modified form (see response above).
The intervals used to classify parental education correspond to the length of schooling in the different school types in Germany. As indicated in the manuscript, the classification is based on the parent with the most total years of schooling.
The reviewer is right in pointing towards the potential of using more intervals to classify breastfeeding.
Urban/rural was defined according to the national German classification of urban and rural.

Results

Table 1. It is clear if the characteristics are separate by sex, male and female. Mean age, weight and BMI does not make any sense if we joint males + females and all the age groups; what is the meaning of n% of persons in the house? Why not define groups? Overweight and obesity should be present separate for males and females, also; is father education or mother education, or both? Again, what is the meaning of mean of single child, living space, passive smoking; this should be present as categorical variables and not means.

Response:

The aim of the paper is to investigate the influence of a number of endogenous and exogenous variables on the odds of overweight/obesity. Therefore, the variables are presented simultaneously in the table. Male/female as well as age are already being taken into account in that age- and sex-specific cut-offs are applied.
The reviewer suggests to define groups from the number of persons living in the household. We decided to use a variable with nine values (1-9), so this is a grouping in a certain sense. It is outlined in the manuscript that the educational variable is derived from the parent with most years of schooling. Contrary to the reviewer’s statement, single child, living space and passive smoking are already presented as categorical variables in table 1.

Table 2. What is the difference of p value with stars and without? Why not used * p< 0.05; ** p < 0.01; *** p < 0.001?

Response:
The meaning of the stars is clearly explained underneath table 2. It has nothing to do with significance.
Table 4. Were all the OR statistically significant?

Response: All those odds ratios where the confidence interval excluded unity were significant in table 4. It was decided to report estimates rather than p-values as the reporting of p-values conveys little information and is based on arbitrary conventions. Reference: Gardner MJ, Altman DG: Confidence intervals rather than P values: estimation rather than hypothesis testing. Br Med J (Clin Res Ed)1986 Mar 15;292(6522):746-50.

Reviewer 2

**Reviewer's report**

**Title:** Predictors of overweight and obesity in five to seven year old children in Germany: results from cross-sectional studies

**Version:** 1  **Date:** 21 December 2007

**Reviewer:** Michael Toschke

**Minor essential revisions:**

1) Although it might be some additional work, I wonder if the authors want to generalise their article and findings for an international readership by rewording and adding some international literature rather than focusing on German readers.

Response:

We are grateful for this suggestion, and have decided to generalise the article a bit more for an international readership, as the readership of BMC Public Health can indeed be expected to be international. We have added international literature both to the introductory paragraphs as well as to the discussion section of the manuscript.

2) **Statistical analysis:**

a. Did the authors test for multicollinearity? I would especially assume the smoking variables to be collinear as regards contents. Therefore, including collinear (if so) smoking variables (like current smoking and smoking in pregnancy) into the same model might yield biased effect estimates and might explain the low effect of smoking in pregnancy. The same holds for other covariates, e.g. for birth weight and smoking etc.

b. Did the authors examine any interactions?

Response:

a. The reviewer points out the important issue of multicollinearity. We have reported both univariate and multivariate estimates. A positive correlation of variables such as the smoking variables would result in an underestimation of the effect size in the multivariable models. The univariable effect sizes would potentially represent overestimations. It is therefore likely that the true effect sizes would lie somewhere between the univariable and multivariable estimates. The issue is now discussed in a paragraph on limitations in the discussion section.

b. The primary aim of the study was to investigate the influence of a number of factors on overweight and obesity by performing predictive analysis, rather than to examine the
influence of single factors, which are potentially confounded and which interact with other factors. In the latter case a systematic approach examining confounding as well as effect modification would have been very important.

3) Limitations/methodological considerations: I would appreciate a discussion on methodological considerations addressing issues such as reverse causality (in particular temporal relationship) and measurement precision of exposures.

(i) Reverse causality

Response:
The reviewer rightly highlights that cross-sectional investigations cannot provide definite information with regard to directions of causality between exposures and outcomes and that reverse causality might be an issue. This is now being discussed and included in a broader paragraph on methodological limitations at the end of the discussion section.

(ii) Measurement precision

Exposure assessments were based on self-reports, which might be inaccurate. Information on some exposures had to be recalled retrospectively (e.g., birth weight) and might be prone to recall bias. Also, reports might be distorted due to social desirability (e.g., smoking during first three years of the children’s life). The issue is now being discussed in the last paragraph of the discussion section.
Reviewer's report

Title: Predictors of overweight and obesity in five to seven year old children in Germany: results from cross-sectional studies

Version: 1 Date: 12 January 2008

Reviewer: Francisco B Ortega

1) The manuscript is in general well written and interesting. The fact that more than thirty five thousands of young children have been assessed in relation to overweight and obesity needs to be recognised. In addition a substantial number of potential factors have been studied. Some strengths and limitations can be highlighted in this study.

2) The terms â"normo-weightâ" or â"norm-weightâ" are commonly used in the literature, however there not strictly correct, since this group includes also to those individuals with an undesirable low weight, underweight people. Cole has recently published the international sex- and age- specific cut-offs for defining under-weight in children. Given the large sample size included in this study, as well as, the number of determinant factors studied, the paper could substantially increase in interest and originality if three weight status categories were compared with a real normoweight group: underweight, overweight and obesity. It will be one of the first papers reporting this valuable and novel information. This is just a possibility, leave this for another paper is the alternative option. The editor responsible for this manuscript will decide if this major modification should or should not be made.

In case that the editorial decision is make it, the main changes to be performed would be:

- Potential title: Underweight, overweight and obesity in five to seven year old children in Germany.
- Table 1 should include a line with the prevalences for underweight.
- A new table should be added showing the same information than table 2 and 3, but for underweight.
- And a new column should be included in table 4.
- Introduction and discussion should be adjusted to the new approach.

Response: The reviewer highlights an important and often neglected issue in drawing attention to the importance of studying underweight. However, it was not the intention of this paper to study underweight, but to study overweight and obesity. A decision was therefore taken to leave the issue of underweight for a separate analysis, should resources be available.
Limitations:
- Although the aetiology of obesity is rather complex, a simple physiological factor is the main responsible for the energy balance. Energy intake and energy expenditure are therefore, two key elements when studying obesity. Although the authors indicate this limitation in their paper, that does not solve the problem. It would be very interesting to see which factors remain significantly associated with obesity, after controlling for dietary factors and/or physical activity. Unfortunately, I guess that these data are not or could not be available, and then there is nothing to do with.

Response: The reviewer rightly underlines the importance of energy intake and energy expenditure as key elements when studying obesity. Unfortunately, we do not have data available in this study relating to dietary factors and/or physical activity. Interestingly, there is conflicting evidence in the literature with respect to diet and physical activity in the aetiology of obesity, for instance dietary habits were not significantly related to overweight/obesity in the study from Kiel/Germany (Danielzik et al.) Another example is the Portuguese study by Padez et al., in which energy intake was not significantly associated with overweight/obesity.

- From my point of view, introduction and discussion are too focussed in German studies. The authors should attempt to transfer a bit these sections from a national to a more international approach. For instance, if breast feeding has shown to be associated to overweight/obesity, it does not really matter were the data are from, since are biological associations. I suggest comparing their own results with the most relevant studies in the field, including comparison with data from different countries.

Response:
It is certainly of value to expand the scope of the discussion of our results. We have now included in the manuscript a number of new international references, both in the introduction and the discussion sections.

MINOR COMPULSORY REVISIONS

Abstract:
It contains the relevant information and is well-structured.

Background:
- As mentioned above, a more international perspective should be used.
- The second paragraph of the introduction, specially the first sentence it is strangely written, I suggest to re-writing it and to include some references supporting the statement. In the second sentence, it is indicated previous studies##, do you mean the two mentioned above or in general? At any case, please, include here which studies you are referring.

Response: The introduction has now been substantially modified.

- The last sentence of the third paragraph. Please, give some references of the studies that have shown these contradictory results concerning breast feeding.
Methods:
- First section. The sentence “Height was measured and underwear should be placed in the following section.”

Response: The sentence has now been moved to the following section.

- I find unnecessary the whole explanation about the Cole cut-offs. I suggest to just indicate that the international cut-off points suggested by Cole et al., were used to classify childhood overweight and obesity.

Response: The sentence referring to the range of the cut-off points has now been moved to the abstract, as requested from reviewer 1. There is now only one sentence left in the methods section relating to the cut-off points as suggested by Cole.

- The exposure variables section is well-written. Change the sentence indicating “1, 2, 3, 4, 5, 6, 7, 8, 9” for something more simple like: “The possible answer ranged from 1 to 9 or more.”

Response: The sentence has been changed according to the reviewer’s comments.

- Preterm delivery. Please, define starting from which week of gestation was a child considered to be preterm.

Response: The information on “preterm delivery” has been obtained from questionnaires and is hence a self-report, as indicated in the manuscript.

Results:
- The section is well written and includes the important information.
- I have not find any information about the differences between participants and non-participants in this paper or in the previously published one (ref. 14). Is it this information available. Usually, participants have a higher socio-economic status and a lower BMI than non-participants, affecting this to the prevalences of overweight/obesity (see Wennlof et al. [1] as an example). If the data are available, include some information, if not, add a sentence in limitations.

Response: In contrast to the reviewer’s comment, a non-response analysis had been carried out in the previously published paper, comparing children from whom BMI was available with those for whom BMI was missing (Apfelbacher et al. 2008, table 6). In this paper, in the section on limitations of the study, a paragraph on non-response is included, discussing non-response in the study in general.
Discussion:
- See commentary in #limitations above.
- Give some references supporting that PA and diet are important factors related to obesity. For the first one, you can use for instance our paper recently published in IJBNPA [2].

Response: The paper reporting results from the European Youth Heart Study is now being referenced in the introductory and the discussion section.

- Page 13, second paragraph, second sentence: "The study in East German children study found... It seems to be something wrong, may be: An East German children study found..."

Response: “The study in East German children” was replaced by “A study in East German children”, according to the reviewer’s comment. “Too” has been removed according to the reviewer’s comment.

Conclusion:
- Remove #in conclusion in the second line.

Response: “In conclusion” in the second line was removed from the manuscript.

Table 4.
- Add a comma to the sample size #N=26,777 instead of #N=26777

Response: A comma has been added according to the reviewer’s comment.

References used:


Further changes:

The prevalence trend article that was cited was published in the meanwhile, and is now fully quoted (reference 14).