Author's response to reviews

Title: Impact of physical activity on energy balance, food intake and choice in normal weight and obese children in the setting of acute social stress: A randomized controlled trial

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Author's response to reviews:

25th September 2014
Dear Editor,

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Impact of physical activity on energy balance, food intake and choice in normal weight and obese children in the setting of acute social stress: A randomized controlled trial.

Antje Horsch, Marion Wobmann, Susi Kriemler, Simone Munsch, Sylvie Borloz, Alexandra Balz, Pedro Marques-Vidal, Ayala Borghini and Jardena J Puder

Many thanks for your letter dated 17th July 2014 including the comments of four reviewers.

We have revised the above manuscript in response to the helpful comments of the reviewers and would like to resubmit it for publication in BMC Pediatrics. In the following, I will list each of your comments and in turn demonstrate how we have addressed them. Furthermore, we have highlighted all the changes in the manuscript in red.

I believe that the revised manuscript comprehensively addresses all of the concerns raised by the reviewers and trust that you will now see it fit for publication. Furthermore, I would like to thank all three reviewers for their valuable comments.

Yours sincerely,

Dr. Antje Horsch
Reviewer 1: Jillon S Vander Wal

Major Compulsory Revisions

1. Study design, first paragraph: Parents were advised to provide their children with a fruit snack at 3 pm (line 151). Please indicate whether the provision of a fruit snack was verified and if so, whether there were any differences among the groups. If not verified, please include as a study limitation.
   Response: The provision of a fruit snack was not verified. This was added as a limitation in the discussion section (p 18, last para).

2. Study design, second paragraph: Please add an explanatory sentence for the difference in target heart rates between the NW and OV/OB children (line 167). Also, please add an explanatory sentence or clarify how the PE specialist ensured that children's perceived exertion was rated as “somewhat hard to hard” (line 168).
   Response: Children’s heart rate was monitored throughout the intervention by a polar watch, aiming for a heart rate of 140 beats/min for the NW and 160 beats/min for the OW/OB children in order to correct for the increased oxygen consumption, i.e. higher exercise intensity in the OW/OB group at a given work output. (Davies et al., 1975) (p 9, first para). The PE specialist also ensured that the children’s perceived exertion was rated as “somewhat hard to hard” based on a repeated check every 5 min that rating on the categorized Borg scale was between 4 and 6 (Borg, 1982) (p 9, first para).

3. Study design, general comment: Please clarify whether children participated in each phase of the experiment individually or in groups.
   Response: In each phase of the experiment, children participated individually (see p. 8, line 159: ‘individual appointments took place...’).

4. Study design, TSST-C: Please comment on whether there was any type of manipulation check and if not, please mention as a study limitation.
   Response: We added the following sentence to study limitations: ‘No manipulation check regarding the TSST-C was carried out in this study, although the standard and widely published protocol was adhered to.’ (p 18, last para)

5. Measures, food intake and choice: Please clarify whether these are common children’s foods in Switzerland (cherry tomatoes?); also, if there are other common international terms, please place these terms in parentheses, e.g., perhaps crisps are similar to potato chips; biscuits are similar to shortbread cookies, etc.
   Response: Yes, the foods that we used are common and popular children’s
foods in Switzerland. They were recommended by a pediatric dietician. Following your suggestion, we have added common international terms (p 10, last para).

6. Measures, parents’ questionnaires: Perhaps add a parenthetical statement as to why migrant status is important to measure, particularly in terms of an association with obesity or physical activity. Also, with regard to the ascertainment of SES, please clarify how the two SES scores were combined to form a unitary metric.

Response: We inserted a sentence explaining that children from migrant families are at a higher risk of obesity than their native counterparts (p 12, first para). The SES was calculated by averaging 4 variables estimated on a 4-point scale: academic or professional training and job position, separately for the mother and for the father (See Pierrehumbert, Frascarolo, Bettschart, Plancherel & Melhuish, 1991 for more details).

7. Measures: For the Conners’ 3, please provide data on the use of the Conners’ 3 in Europe if available or list as a limitation. For the DEBQ, please clarify what is meant by the term “adapted.” For the APQ, please provide data on the use of the APQ in Europe if available or list as a study limitation.

Response: Conners’ 3 and the Alabama Parenting Questionnaire have been officially translated into French and both are widely used in Europe (e.g. Farré-Riba & Narbona, 1997 for Conners’ 3; Essau et al., 2006 for APQ). The DEBQ has been translated and validated for French Language use (Lluch et al., 1996; Strien & Oosterveld, 2008). The term “adapted” was thus misleading and has been removed (p 11, para 3).

8. Analyses: It would appear that a multivariate approach may be more favorable for some variables, such as the seven subscales of the APQ or the three subscales of the DEBQ. Alternatively, a correction for multiple comparisons might be in order. As such, there exists the risk for a type I error.

Response: If we correct for multiple comparisons the impact of the APQ variables is no longer significant, but the impact related to the DEBQ subscales (restrained eating) remains unchanged. We added this at the end of the result section (p15, para 3).

9. Analyses/results: One analysis of interest was whether the effect of physical activity on food intake differed by weight status. However, there appears to be no mention of the analysis of interaction effects, nor does this study appear to have sufficient power in this regard. Please indicate if interaction effects were considered and if there was insufficient power, please discuss.

Response: We looked at the interaction effects but did not find any significant effects, as food intake and overall energy balance was lower in PA in both weight
groups (as already discussed in the manuscript). Furthermore, we have insufficient power for these calculations, particularly given all the adjustments we made for confounders.

10. Results, first paragraph: Perhaps clarify the direction of the differences obtained in text (lines 272-273).
Response: We have made those changes accordingly (p 13, last para).

11. Discussion, fourth paragraph: Care should be exercised in the discussion of food choices as children’s food preferences may have affected their choices above and beyond the physical attributes of the foods (i.e., sweet/salty, low/high caloric density). That is, the use of different foods representing these combinations of taste and caloric density may have resulted in different food choices. This idea should be included in the discussion along with the limitation that children’s food preferences weren’t assessed a priori.
Response: We have added such a sentence on p 17, para 1.

Minor Essential Revisions

1. line 48: Please reverse the order of “different” and “12.”
2. line 52 and ff: Please add a space after the numbers and before “kcal.”
3. lines 150, 152: Please add a space between the time and pm.
4. line 157: delete the word “a” after “using.”
5. line 160: Please add a space between “13” and “NW.”
6. lines 277, 278: Please change “amount” to “number.”
7. line 311: Please change 9 to 11 year old children to “9- to 11-year-old children.”
8. line 366: Please add a comma after i.e.
9. References: The titles of some of the references need to be placed in lower rather than in upper case: 4, 13, 31, & 38.
Response: Many thanks for these helpful corrections – we have implemented them all.

Reviewer 2: Berit Heitmann

1. My major concern is the "over-selling" of the importance of the findings. The study is short term and compensation may occur later during the day, or the next day or week, and both the discussion and conclusion sections needs to clearly reflect this.
Response: We agree with this comment and have added the following sentence
2. The authors somewhat mislead the reader making us think their results are representing causal relationships - for instance: 361-362 - the results do not confirm the "role" of... for "development of",
or: 398-399 - this study is not about "risk" of obesity, but a study including obese individuals with not information on change in weight. Please modify text
Response: This is a very helpful comment – thank you. We have made changes accordingly throughout the manuscript.

3. Also from the conclusion the sentence (411-412) ...positive parenting can act as a protective factor preventing... needs to be omitted.
Response: We have deleted this sentence (p 20, para 2).

4. In terms of generalizability we need more information on the selection criteria. We know that (in total) 82 children agreed to participate - but we don't know how many were recruited in total.
Response: Regarding OW/OB children, 52 children were recruited and of those, 38 agreed to participate (as stated on p. 8, para 2). However, the NW children had to opt in in response to an advertisement. It is therefore not possible to know how many parents of NW children saw the advertisement but did not decide to enrol their child in the study.

5. Some discussion is needed in regards to the differential participation among normal weight (26/30) and OW/OB (24/52) and how this may potentially influence results (inflate or attenuate) as well as generalizability.
Response: In order to address this, we added the following sentence: “It is possible that parents whose NW children were particularly interested in sports responded to the advertisement, thus attenuating the group differences.” (p 19, first para)

6. Lines 385-386 and 387-389 we are informed about potential bias - but not on the way such bias may influence the present results (eg lead to attenuation or inflation). Please specify
Response: We made the potential influence of a bias on the results more explicit: ‘Staff who measured the food intake in order to calculate energy balance was not blinded to the participants’ allocation, which might have attenuated the group
Regarding the bias based on the different methods of recruitment, we added the following explanation: ‘It is possible that parents whose NW children were particularly interested in sports responded to the advertisement, thus attenuating the group differences.’ (p 19, first para)

7. Also, in a few places quotes or statements are unsupported by references - for instance line 326-328
Response: Apologies but we do not understand this comment. The lines 326-328 contain the following sentence, which is backed up by two references: ‘In the absence of stress, acute physical activity does not necessarily lead to an increase in food intake or appetite or appetite hormones despite an increase in energy expenditure; it can even reduce short-term food intake [39,40].’

Miscellaneous:
8. Energy is reported with one decimal - a precision that is impossible - please report without decimals
Response: We have corrected this throughout the text (including tables).

Reviewer 3: Janine M Jurkowski

1. Line 76- Define social stress. Consider moving the more detailed description in lines 93-94 up to near line 76.
Response: We have included a brief definition of social stress (p. 5, first para) and moved the more detailed description of social stress, as suggested, to line 76: ‘These exposures, particularly to social stress (negative social interactions and interpersonal relationships that can aggravate social stigma and exclusion), occur frequently in children, and overweight children are especially targeted.’

2. Line 101-102-- Be more specific why ADHD was mentioned in the introduction. Unless they are studying it, discuss how ADHD is relevant to study such as a growing population of children with ADHD. Present data. Otherwise, there is no reason why impulsivity needs to be discussed with mention of ADHD.
Response: We have included two additional sentences and two references that explain why we chose to investigate impulsivity in this study (p 6, para 3).

3. Methods are strong but the fact that the refusals to participate in the OW/OB group is greater (appears to be significantly greater) causes the reviewer to question potential sampling bias. Please address this in either or both the Methods and limitations.
Response: As expected, it was harder to recruit a clinical population of obese children than a healthy control group. We agree with the reviewer – a potential sampling bias can therefore not be excluded. We have added a sentence to the
limitations section accordingly (p. 19, first para).

4. Limitations section needs discussion of issue of sample size. This is a significant limitation. Also, again, the discussion of sample bias.
Response: Both issues are discussed in more detail in the limitations section (p 19, first para).

Reviewer 4: Jana Strahler

1. Not measuring the acute psychobiological stress response is a clear limitation of this study and prevents important insight into biological mechanisms of stress-induced food intake (e.g. HPA axis activity).
Response: In fact, we did measure the acute psychobiological stress response (HPA and ANS responses), which also served as a manipulation check for our experiment. However, given the complexity of the data, we decided to publish this as a separate paper.

Major Compulsory Revisions

2. Abstract results: the wording „decrease“ is somewhat misleading since the authors compared only stress-induced eating after either physical activity or sedentary behavior (and not before and after an intervention). Maybe „lower energy balance after physical activity compared to the sedentary condition“ would be more appropriate. – Please check the manuscript accordingly (results and discussion)
Response: Thank you for this helpful comment. We have changed the wording in the abstract, results and discussion sections accordingly.

3. Hypotheses: From the literature review, hypotheses are expected in a slightly different way compared to their actual appearance. A) a direction of results can be hypothesized - from the literature, one can assume reduced food intake after activity. As well, PRECEDING or ACUTE physical activity would be a more precise operationalization. B) I’m not quite sure why the authors expect an independence of allocation. Since physical activity is introduced as a prevention strategy in obesity and to augment stress-induced eating, a physical activity by weight group interaction should be expected. Please comment and update if necessary.
Response: Following this comment, we have changed hypothesis A) to reflect the suggested direction of the relationship between physical activity and reduced food intake (p 7, first para). Our hypothesis B) is based on evidence (discussed in the introduction) showing that following stress exposure, overweight/obese
children tend to eat more and are likely to choose unhealthy/ comfort food compared to normal weight children.

4. Methods: Did any of the children take prescribed medication? Was any mental disorder reported? Were all children prepubertal – how was this measured? Did you control for physical fitness status/regular exercise (hours/week) - this might moderate the effect of an acute bout of exercise on stress-induced eating (see also discussion)?

Response: As described in the inclusion criteria (p 7, last para), none of the children had chronic medical problems. We asked about the use of medication and found no group differences. We classified the children as ‘prepubertal’ according to their age (7 to 11 years). We did not control for regular exercise but ensured that all children were physically able to perform the acute physical exercise of 30 min in our experiment. Following this comment, we have included the lack of control for the effects of chronic exercise in the study limitations (p. 19, first para).

5. Methods: As the authors state in their discussion, physical activity is not necessarily related to increased food consumption in the absence of stress – a relation that might have also been addressed by the authors. Why did they choose to refrain from investigating a no stress control group? I’m sure this was done on purpose. I would love to understand their decision.

Response: In fact, ideally, we would need 4 more control groups (OW/NW group with or without stress and physical/sedentary activity group with or without stress). Given that OW/AB children are difficult to recruit, this was not possible. However, we could include this as a limitation if the reviewer wishes.

6. Results, 2nd paragraph: I’m not sure what the percentages actually mean. Considering stress-induced eating in the sedentary condition as the „normal (=100%)“ state, do you mean 30/60% reduction from this baseline or reduction to 30/60%?

Response: Following this comment, we have taken the percentages out (p 14, first para).

7. Results, 2nd para: Expenditure and balance results are missing for hypothesis b)

(difference between OW and NW).

Response: We have added these results (that are already displayed in Table 3) to the results section (p 14, first para).

8. Results, last para: Is this association anyhow related to activity group allocation and weight group? Please state.
Response: Based on our hypothesis C), significant group differences regarding impulsivity, eating behaviour (restrained eating) and parenting style (corporal punishment) are already reported on p.14, 3rd para. We did not investigate whether we could find these differences with regards to randomisation group, as this was not a hypothesis.

9. Discussion (Line 338-339): Intervals of physical activity are recommended as a stress coping strategy. This raises the question of a possible impact of physical fitness/regular physical activity where the body adapts to acute bouts of exercise (see also above). This should be discussed (as a limitation).

Response: We have included the lack of consideration of chronic physical activity as a study limitation (see comment 4). However, it has already been well established that physical activity is a very effective stress coping strategy. The novelty of our study is the finding that even a short bout of physical activity of 30 min can make a difference.

10. Discussion (Line 344-345): „decrease in stress reactivity due to physical activity“
– did the authors measure psychobiological stress reactivity? – also relevant as a manipulation check, i.e. did their stressor actually induced stress and can we assume a „stress-induced“ change in eating? As well, especially stress-induced HPA axis activity has been introduced as a possible modulator of food-intake after stress and a pathway to obesity (e.g., Sominsky & Spencer, 2014, Frontiers in Psychology). This mechanism might be included in the discussion as a possible future direction of research.

Response: As we stated in this paragraph, another study had reported a link between higher stress reactivity and higher intake of salty foods. In order to answer your query, we have added the word „psychobiological“ (p 17, first para). Thank you for suggesting this new reference that furthers our argument; we have added it to our discussion.

11. Figure 1: Since nearly all included information can also be found in the text, this figure is not necessary.

Response: Following this comment, we have taken Figure 1 out.

Minor Essential Revisions

12. In their abstract, the authors introduce physical activity as a regulating mechanism of eating behavior – „moderator“ or „influencing factor“ would be more appropriate.

Response: We have exchanged ‘regulate’ with ‘moderate’.
13. As well, please be more precise in your wording – you are interested in ACTUAL or ACUTE physical activity.
Response: We have added ‘acute’ throughout the text.

14. Please mention your social stressor in the abstract (e.g. “(Trier Social Stress Test for Children”)).
Response: We have added this.

15: Abstract: „At the end of the experiment“ – experiment should be stressor.
Response: We have changed this both in the abstract and in the method section.

16: Statistics: chronic eating behavior should be HABITUAL eating behavior
Response: This has been changed throughout the manuscript.

17: Discussion on parenting style (Line 367): Is this true for both groups/allocations?
Please state.
Response: Yes, this is true for both groups/allocations. We have added this clarification (p18, para 2).

18: Next sentence (Line 369): „autonomous“ regulation + „external food cues“– What do you mean?
Response: We have changed this sentence to the following: ‘This is intuitively linked with self-regulation of food intake and there is a lower risk of responsiveness to external food cues, e.g. the sight and smell of food.’ (p 18, para 2)

Discretionary Revisions

19. The introduction comprehensively describes the authors line of reasoning and introduces all variables of interest in a very precise way. Some comments might further strengthen this part. Line 81: physiological mediators should be endocrine, given your examples.
Response: We have replaced ‘physiological’ with ‘endocrine’ (line 82).

20. Line 80-82: Please consider dividing this long sentence.
Response: We have devided this sentence into two (p 5 para 2).

mean in response to physical activity?
Response: We have added ‘following physical activity’ in order to clarify this (p 5, para 2).

22. Line 85: a short definition of comfort food should be integrated.
Response: We have done this: ‘foods with high carbohydrate levels that give emotional comfort’ (p 5, para 2).

23. Line 104: “To what extent ASSUMED impulsivity IN OW“.
Response: We have added a sentence to clarify this: ‘Pre-pubertal obese children already display greater levels of impulsivity compared to healthy-weight peers, indicating that it may contribute to the onset and maintenance of obesity.’ (p 6, para 3).

24. Methods: When describing energy expenditures, providing always the description of behavior first followed by (MET) might ease the understanding of this paragraph.
Response: We changed this accordingly (p 11, first para).