Author's response to reviews

Title: Thyroid function derangement and childhood obesity: an Italian experience

Authors:

Anna Grandone (agrandone@gmail.com)
Nicola Santoro (nicolasantoro@hotmail.com)
Filomena Coppola (menacop@libero.it)
Paolo Calabrò (paolo.calabro@unina2.it)
Laura Perrone (laura.perrone@unina2.it)
Emanuele Miraglia del Giudice (emanuele.miraglia@unina2.it)

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To the Editor of the

**BMC Endocrine Disorders**

Dear Editor,

Following your letter of February, 25, 2010, on behalf of other authors, I’m resubmitting for publication in your Journal the paper entitled “Thyroid function derangement and childhood obesity: an Italian experience.” by Anna Grandone, Nicola Santoro, Filomena Coppola, Paolo Calabrò, Laura Perrone and Emanuele Miraglia del Giudice.

We thank you for the opportunity for resubmitting the manuscript and the referees for finding it of some interest. We tried to improve the manuscript considering all points raised by Reviewers. Particularly, you will find a detailed reply with the answers to the reviewers comments.

We really thank you for giving us this opportunity and, although we are aware that it does not guarantee the final acceptance, we hope that the paper has improved enough to deserve to be published in the BMC Endocrine Disorders.

We added an Authors’ contribution section. We do not have any private funding to declare.

Regards

Anna Grandone

Reviewer 1 (Dr Hochberg)

Major comment: In the absence of a control group, it is impossible to conclude that obese subjects have high TSH levels.

We agree that our statement about the high prevalence of isolated hyperthyrotropinemia among obese children would be more strong with a control group.

Anyway our findings resemble the results of several other controlled studies in obese children that are quoted (references 4-7). Moreover a recent study on BMC Endocrine Disorders, by Kapelari at al (BMC Endocrine Disorders 2008, 8:15, reference 20) in which the aim of the study was to establish reference intervals for serum TSH, fT3, and fT4 from birth to 18 years, only 32 out 1209 normal weight children (2.6%) had isolated elevated TSH levels, confirming a previous study about the prevalence of isolated subclinical hypothyroidism in children and adolescents (BMC Pediatr. 2006 Apr 19;6:12. Wu T et al. reference 19). Anyway we added a sentence stating this limitation of our work (see page 9 line 5-6).
For the remaining analysis and considerations of our study, of course, it is not needed a control group as comparisons are all performed in obese subjects (i.e.; among obese subjects with and without hyperthyrotopinemia and among obese subjects with and without weight loss).

Minor comment

1. The fact that the study was conducted in Italy is of no importance to the Title or Abstract.

According to the considerations of Reviewer 3 we think that to outline the different ethnic origin of our sample compared to the children till now investigated (e.g.; the German children investigated by Reinehr) could be of interest for the readers.

Subjects with BMI Z score 1.2 can hardly be defined as obese.

This was a typing mistake which we have emended in the new version of the manuscript. The range of BMI z-score is 1.8-5.8 (see new version of Table 1).

Reviewer 2 (Dr De Pergola)
The results and the conclusions of this study in children are strictly in line with those of the paper performed in obese adults from De Pergola et al, that should be cited (De Pergola G, Ciampolillo A, Paolotti S, Trerotoli P, Giorgino R: Free triiodothyronine and thyroid stimulating hormone serum levels are directly associated with waist circumference, independently of insulin resistance, metabolic parameters and blood pressure levels in overweight and obese women Clinical Endocrinology, 67: 265-269, 2007)

We quoted the suggested article (page 10 line 17 and reference 31)

Reviewer 3 (Dr de Boer)
We reviewed the article with an English speaker to improve English.

Minor comment

1. Page 3, paragraph 2, line 1: “Several studies” is mentioned, but only one study is referenced. The authors should provide further examples.

We quoted some references more and added reference 6 (see page 3, line 13)

2. Page 4, paragraph 1, line 3: Criteria are listed for exclusion of subjects. The authors should state the number of subjects that were excluded—either here or in the results section.

We added the number of subjects excluded as requested by the reviewer (page 4 line 5-6)

3. Page 6, paragraph 1, line 3: Weight reduction was considered at least a 0.5 decrease in BMI z-score. Do the authors have a reference to cite that lists this as a definition of weight loss?

In the results section, we quoted the article by Reinehr et al (Arch Dis Child 2002, 89:419) which shows that weight loss is associated with an improvement in the atherogenic profile and in insulin resistance, only if the SDS-BMI decreases by at least 0.5 (see page 8 line 12). We added this citation (see page 6 line 4).
4. **Page 6, 5th line from the bottom:** Linear regression model was performed…were levels of leptin also examined in the regression model for the sub-group of patients in which this was tested?

We adjusted the linear regression model also for leptin in the subgroup in which it was tested but its contribution was not significant. We added this information (page 6 lines 4 and 5 from the bottom).

Reviewer 4 (Dr Javier J Salvador)

1. *I will recommend to display leptin levels after weight loss in the groups showed in Table 3, and to established if there was a correlation between changes in leptin and those in TSH/T3 values following weight loss.*

We agree it would be interesting, but unfortunately we do not have these data as we did not determine leptin levels after weight reduction.

2. A comment on the possible role of changes in D2 deiodinase activity at pituitary level, which controls the feedback of T3 on TSH secretion should be included in the Discussion.

As suggested we added a comment and a reference (see page 10 lines 5-8, reference 28)