Reviewer's report

Title: Obesity in children with Down syndrome: The role of some hormonal parameters "a comparative study"

Version: 1 Date: 17 April 2012

Reviewer: Kate Steinbeck

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This is cross-sectional study of obese and non-obese children with and without Down syndrome which allows for a number of comparisons. As it is a one time point study, it does not allow for delineation of possible roles in the pathogenesis of obesity of the hormones studied OR for other biochemical data studied. This goal would require a prospective study. All that this method can provide are correlations, as have been reported. The methods are generally well described and obese and non-obese groups are discrete from one another. I would like it stated how pubertal development was determined - ? direct observation or self report. The biochemical analyses are standard. The statistics are appropriate. Generally the manuscript adheres to the relevant standards for reporting and data deposition.

The results are clearly laid out in Table 1. The text for the results is somewhat repetitive and it would perhaps be better to highlight the differences between OD and NOD groups. Generally the expected differences between O and NO are present.

MAJOR COMPULSORY REVISIONS

1. The word role in the title should be removed – the study is a straight, adequately controlled description of biochemical and hormonal findings.

2. CHANGE THE AIMS: The Aim of the study is to evaluate obesity-related hormone in prepubertal children with Down syndrome and to delineate their possible roles in the pathogenesis of obesity. The aim is clearly stated after a brief but clear introduction but the study cannot achieve the second part of the Aim.

3. DISCUSSION AND CONCLUSION NEED FULL REWRITE: My major concern about this study is that the discussion is too long, over-interprets the data and makes unsupported conclusions. It could be significantly improved if it was briefly stated what are the novel findings in this cross-sectional study and what simply replicate the findings of others. In other words what does this study contribute. Since leptin is not generally considered to have a major role in the pathogenesis of obesity, except in rare genetic deficiency or resistance states this should be withdrawn from the conclusion. There is a greater degree of insulin resistance in the DS but this is hard to interpret without body composition data or information distribution of fat with a waist circumference. The CK results are certainly
interesting and CK is elevated in OD and NOD and some of the DS clearly have sub-clinical hypothyroidism. Has this been reported in DS before and could there be another explanation. The lowest FT4 is actually in the OND group. As discussed above the limitations have not been clearly stated.

MINOR REVISIONS

4. The authors need to confirm that the median FT4 level for OND of 5pmol/L is correct as this would surely be in the hypothyroid range. The CK results in DS are interesting. The correlations in Table 2 would be easier to read if significant correlations were bolded.

5. There are some minor grammatical issues. E.g. ‘did not’ should replace ‘didn’t’ (line 13 of discussion – NB it would have been useful to have lines numbered); Line 4 of the discussion ‘increases’ rather than ‘increase’; Line 13 of discussion should read ‘an unaffected sibling’.

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I DECLARE THAT I HAVE NO COMPETING INTERESTS