Reviewer’s report

Title: Rapid Increase of Overweight and Obesity among Primary School-aged Children in the Caribbean; High Initial BMI is the most Significant Predictor

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Reviewer: Tony Fitzgerald

Reviewer's report:

General Comment

The authors consider predictors of changes in BMI among primary school-aged children in the Caribbean. The study included children aged 6 to 10 with a follow-up of 18 months. The change in children's BMI was calculated by "subtracting the sex-age-specific median BMI using the WHO reference from the measured BMI from baseline and follow-up measures." The authors cite Tim Cole (reference 25) who recommends change in BMI, as opposed to change in BMI z-score, as a measure of change in adiposity. The model used presumed that any association between BMI change and age, height for age (HFA) and baseline BMI z-score was linear.

There is not a consensus, in my opinion at least, on how best to adjust observed changes for baseline. Some recommend adjusting for baseline by including baseline as a covariate, as the authors have done, and others recommend subtracting baseline but then not including baseline as a covariate. Clearly the second option would not allow the authors consider the analysis presented in this paper. I recommend the authors frame their analysis/discussion in the more general context of growth patterns, in particular how BMI changes with age. It would benefit the paper if the authors described 'typical' (adjusted) BMI growth patterns over the age-range of interest. Are they linear for example? Does the association between change and baseline suggest that growth is not linear? Failure to adequately adjust for baseline BMI, when modelling change in BMI, can lead to spurious associations with factors associated with baseline BMI. As the authors demonstrate in table 2, there is a clear association between HFA and baseline weight status. It may seem that baseline BMI-Z score is independent of age but this may not be the case. In short, I recommend an expanded discussion of growth patterns that should inform the interpretation of results.

Specific Comments

1. It's not clear whether the authors considered differences between countries or adjusted for country in subsequent analysis?
2. Table 1 describes demographic data for children and caregivers at baseline by weight categories of the children. Given that the results presented are descriptive, without an underlying hypothesis, I would not include p-values in this table.

3. Table 2 describes children's weight status at baseline and follow-up and highlights a movement toward higher obesity levels based on BMI Z-score defined categories. The authors present a p-value but it is not clear what test was used.

4. Table 3 compares dietary intake in those who don't move up a weight category and those who become overweight or obese. I recommend the authors reconsider this comparison as those not moving up a weight category will include both those who maintain a healthy weight and also those who remain obese. Clearly children who are obese at baseline cannot move up a weight category. It may be more informative to create groups based on a magnitude of the changes in BMI. For example, Jennifer O'Loughlin et al (reference 15 in the submitted paper) dichotomised subjects based on the highest decile of BMI change.

5. Table 4 presents the results of a multiple linear regression analysis for the change in BMI. We are told (page 6) that the model included baseline BMI z-score, baseline HFA z-score, age, sex, energy intake, misreporting of energy, food insecurity and caregiver BMI. However, the results presented in table 4 includes only "those variables that had a significant impact on the change in BMI", age, baseline BMI z-score and HFA z-score. The authors should clarify how the model was reduced to 3 factors.

6. Table 4 presents beta coefficients for age, BMI z-score and HFA z-score. The table should include units for age, years I presume? It appears that the change in unstandardized BMI (after subtracting medians) is the outcome variable. The inclusion of standardised BMI as predictor makes the interpretation of the final beta coefficients difficult, for me at least.

7. Table 4 considers a "multiple regression model of association with adiposity in children". From the methods section it appears that the dependent variable in table 4 is the change in BMI? Table 5 looks at the "change in adiposity among children". The footnote states that "adjustment for age was done by subtracting the sex-age-specific median BMI". It is not clear whether tables 4 and 5 have the same dependent variable.

8. Table 5 is interesting and in my opinion gives a better insight into the association between baseline BMI and BMI change than table 4. It is clear that the change in BMI depends on baseline BMI. The association is presumably more than could be explained by regression to the mean. From table 5, BMI change is positively associated with baseline BMI. From table 1, HFA is positively associated with baseline BMI. Based on tables 1 and 5, it is not
surprising that both baseline BMI and HFA are associated with BMI change. I recommend the authors consider table 5 before table 4.

9. The authors state (final paragraph of discussion) that "misreporting of energy intake was assessed in our study and controlled for in the regression analysis". It is not clear how misreporting was controlled for in the final analysis. Table 4 does not include a measure of misreporting. Presumably it was not statistically significant, but the results presented should not be described as adjusted for misreporting if this term is not included in the final model.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Unable to assess

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