Reviewer’s report

Title: Four years' follow-up changes of physical activity and sedentary time in women undergoing Roux-en-Y Gastric Bypass surgery and appurtenant children

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Reviewer: Adam J Reid

Reviewer’s report:

The opportunity to review this manuscript is much appreciated and the work of Dr. Berglind et al. is interesting indeed. Overall, the study is presented as a descriptive study with subject selection variability, lack of statistical power, data collection issues, and absence of a control group. Regardless, the aims of this study are clearly defined. One of the aims of the study is to objectively evaluate physical activity changes in subjects undergoing weight loss surgery and their children, utilizing accelerometer data over an extended period of time. Furthermore, the study assesses physical activity guideline achievement in women and children after maternal weight loss surgery. It is felt that the aims of this study are achieved with appropriate consideration of the above limitations.

Subjective physical activity data is noted to contradict objective activity data in this patient population and the challenges associated with obtaining sophisticated accelerometer data is illustrated. As suspected, great variability in objectively measured activity levels among weight loss surgery patients is presented. This inconsistency may also be identified when evaluating for a potential halo effect for bariatric surgery. The activity of the subjects' children is evaluated in this unique opportunity to observe behavioral changes that may develop due to frequent engagement and interaction with a weight loss surgery patient, but it is difficult to apply a comparative conclusion. It is noted in the discussion that objectively measured physical activity decreases with age in children starting at the age of five and it may be worthwhile to mention this previously noted decline in the conclusion.

A few questions for the authors. Do the objective physical activity and sedentary times presented follow the norm for the age changes presented in this study? Perhaps the identified physical activity decline among patients' children is less than that observed in the general population thus potentially supporting a halo effect. Is there any correlation between the patients and their own children? In other words, how does the objective physical activity data of patient "X" compare to the data gathered on her own child "x"? Do patients who meet the current physical activity guidelines have children who meet their physical activity guidelines? Answers to these questions may not be achieved with this study but could provide a path to support the halo effect with objective data in the future. Your reflection is much appreciated.

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?
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No

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