Reviewer's report

Title: Physical activity, screen viewing and obesity status in a nationally representative sample of Maltese youth

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Reviewer: Patrick Bergman

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In this study the authors examines the Physical activity, screen viewing and obesity status in a nationally representative sample of Maltese youth. The study has some strengths such as objective assessment of physical activity, measured weight and height, and a nationally representative study sample, there are however several shortcomings that needs to be addressed.

Major Compulsory Revisions

1) The authors fail to motivate in the background why screen time should be monitored. This is an important issue since they later aim to examine the association between screen time and obesity. Moreover, even if there have been substantially reported regarding the positive relationship between screen time (especially tv viewing) and obesity the validity of screen time or tv viewing is very low to non significant (compared to accelerometers, please see: http://www.ncbi.nlm.nih.gov/pubmed/22544913). This is an indication of that it is not the inactivity (as assessed by screen time) per se that is responsible for the development of obesity but rather something else, eg snacking during tv viewing. The authors do not recognize this and it would be interesting to read such an discussion.

2) It is very interesting that the authors is not discussing the negative association between accelerometer determined inactivity and obesity, a finding that goes in the opposite direction compared to the theory that inactive children becomes obese. Instead the authors focus their discussion on self reported screen time which at best can be viewed as a proxy for total inactivity. It is not clear to me why this approach is chosen.

3) There are many reasons to believe that (lack of) physical activity is not a major contributor to the development of obesity (for example see this meta analysis http://www.ncbi.nlm.nih.gov/pubmed/21383837). This is also observed but not acknowledged in the present study. For example:

a) The authors claim that the Maltese youth are among the fattest in Europe yet they seem to be as active as many other as seen when the authors compare between countries.

b) Given that the girls are much less active compared to boys they should also, according to the theory that low levels of physical activity is linked to obesity also have a higher prevalence of obesity, but in this study the opposite is seen.
4) It can be discussed if the proper metrics from the accelerometers when modelling associations between PA and obesity is used. It have been shown that the number of minutes of MVPA is reduced in obese subjects while the activity energy expenditure is the same between obese and normal weight subjects (http://www.ncbi.nlm.nih.gov/pubmed/12399263). Thus a better measure would perhaps be to use one of the published equations for determining energy expenditure based on accelerometer counts and compare that across BMI categories.

5) Experience tells us that physical activity data is often positively skewed. This may cause problems when using linear models such as ANCOVA. There are some indications that this is also the case in this particular study (based on the reported means and SD’s). How was this examined, for example what did the distribution of the residuals looked like?

6) There is a poor match between the research aims and the conclusions. In the conclusions only conclusions based on the first aim is reported. I would have expected that at least one conclusion that corresponds to each of the aims is reported.

7) The authors compares their data with data from other parts of the world but I lack the reasoning why those particular studies were chosen. There are a plethora of similar studies from around the world and of particular interest is the International Children Accelerometer Database (ICAD).

8) When reporting prevalence of physical activity based on accelerometer assessed physical activity the cut point chosen to define at least moderate intensity is important. A low cut point will provide higher prevalence numbers while a high will give low. Therefore it is important to define why the particular cut point was chosen and to discuss the potential consequences of such a choice. I also recommended to use several different cut points to increase comparability between studies.

8) The authors mentions that accelerometers are not able to capture behaviours, i.e. what the subjects are doing while being physically active. This is only partly true. Modern methods using pattern recognition analyses such as artificial neural networks are training computers to classify behaviours based on accelerometer readings.

9) Not being able to accurately determine the energy balance (energy intake vs energy expenditure) is of course a major limitation given the research aims in the present study (physical activity and screen viewing across BMI status). This should be acknowledged in the discussion

Minor Essential Revisions

1) In the abstract it can be read that BMI was assessed, but strictly speaking BMI is calculated from measured height and weight. I suggest to rephrase.

2) Given the inherent difficulties of measuring physical activity it has been
recommended to use the word "assess" instead of "measure". I suggest to rephrase.

3) The third aim "How do their physical activity and screen-viewing patterns differ by weight status (adjusting for socioeconomic status)" is strictly speaking wrong it should rather read "How do their physical activity and screen-viewing patterns differ by BMI status (adjusting for socioeconomic status)". I suggest to rephrase.

4) There are two number three aim in the background.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**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