Author's response to reviews

Title: Physical activity, screen time and obesity status in a nationally representative sample of Maltese youth with international comparisons

Authors:

Andrew Decelis (andrew.decelis@um.edu.mt)
Russell Jago (russ.jago@bristol.ac.uk)
Kenneth R. Fox (K.R.Fox@bristol.ac.uk)

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Author's response to reviews: see over
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Dear Dr. Clemes,

We would like to thank you for the reviews of our paper *Physical activity, screen viewing and obesity status in a nationally representative sample of Maltese youth*. In light of the comments we have amended the paper and highlighted the changed sections in yellow. Please find a point by point response to the reviews in the attached file.

Sincerely

Andrew Decelis
o.b.o co-authors Prof. Russell Jago & Prof. Kenneth Fox

(Reviewers: Dr. Rezende, Dr. Onywera & Dr. Bergman)

**Reviewer 1:**

**Major Compulsory Revisions:**

**Introduction:**

1. *The research question posed by the authors are relevant. However, much of this question has been answered in a previous work published: Decelis A, JagoR, Fox KR. Objectively assessed physical activity and weight status in Maltese 11-12 year-olds. I understand that there the previous article was limited to 11-12 years old children. In this sense, a better justification is needed for the arguments based on existing data*

The first study was a pilot study that allowed the refinement of instrumentation and to evaluate the efficacy of the line of study. The critical difference between the present study and the pilot is that it engages a large nationally representative sample (28.6% of full population). This was necessary in order to accurately characterise physical activity and obesity in the full Maltese population of 10-11 year olds and allow international comparisons. This larger sample also provided statistical power to investigate sub-population differences on key variables. An additional difference was that these children were a year younger and attending their final year of primary schooling before the transition to secondary schooling. The pilot results (which included some qualitative work) indicated that the study of a slightly younger age group might be more revealing and also represented a period of stability. We have briefly indicated this in the introduction (Ls 81-85) to avoid further reactions that these two studies are the same.

2. *I do not understand that the comparation of physical activity and screen-viewing levels between countries is a original contribution of the paper. I would recommend the authors to move to the discussion section (including table 3 as a supplementary file). If the authors trust that it should be kept in as a result, a detailed description of the search strategy and selection of the studies should be*
described in the methods section.

We feel this is a very valuable part of the results. Adding to previous research this is allowing us to establish the degree to which obesity and overweight in Maltese children is very high in comparison to other countries. Once this is established it provides a strong case for intervention in Malta but of more significance to the wider research community, it gives grounds for closer study of this group of children to see what the main causes of these high rates of obesity are. In this paper we chose to investigate the associations between physical activity, sedentary time and weight status groups. We have therefore retained this section of the paper. We take the point that the strategy for identification of suitable studies from other countries that have comparable data is important and have added this to the methodology (Ls 140-142).

Methods:

3. Eligibility criteria of selection of participants should be described.

School and classes were the units of sampling with all children from each selected class being invited to participate. There were therefore no additional criteria for each child. We have made this clear in the methods section (Ls 101-103).

4. Validity and reliability of questionnaire data should be described (if available). If this data is not, it should be talked into account in the interpretation and limitations of the study.

We have added an explanation about the development of the questionnaire and its reliability in the methodology (Ls 110-112), and added a comment in the section of the discussion dealing with limitations (Ls 320-322).

Results:

5. To present a Table with general sociodemographic characteristics would be useful for international readers.

We appreciate that a table featuring sociodemographic characteristics of the sample is often featured. We have already presented basic demographics including gender and age in text, and physical activity, obesity and screen viewing are featured in the other tables. The only further variables we have are socio-economic score, school type and geographical region and these are not really of value for international comparison so we have not provided a further table. If the editor felt it important then we would be willing to take guidance.

6. Even though 901 children provided consent to participate in this study (80% of response rate), accelerometer and weekend data present 72% and 68% of response rate, respectively. The authors should take it into account in the interpretation of results. It would be useful (if available) to compare any available
data (e.g. socioeconomic status or other sociodemographic characteristics) between respondents and non-respondents. Reminding the authors that the threshold for minimally acceptable response in prevalence studies should be set at 70% as long as the report shows that respondents and non-respondents, and/or the study sample and the target population, have similar importante sociodemographic characteristics.

Comparison between consenters and non consenters was not possible as non consenters did not provide any data and there are no established national norms for comparison. We investigated the differences in characteristics between consenters providing accelerometer data that met our inclusion criteria and those not providing full data using t-tests and chi-square tests. Results indicated significant differences in socioeconomic status but no differences in gender, region or BMI category. We added a sentence in the results section indicating this (Ls 162-165)

7. The article should include descriptive values (absolute numbers, sd, and Prevalence’s) in the overall sample.

We originally left these additional figures out of tables as they were already quite dense. However in order to address the this reviewer’s comment we have added them to table 1, 2 and 5 where totals were not included.

Minor Essential Revisions:

8. Screen-viewing or Screen-time? Both terms could be used, but I would recommend to keep screen-time all over the paper.

‘Screen viewing’ has been changed to ‘screen time’ in the title and throughout the paper.
Reviewer 2:

1. Minor Essential Revisions:

   a) While I found the title to be clear, I wonder whether the authors are interested in screen viewing by the target population or the time taken by the Maltese youth on screen. There is need to reconsider the use of the term 'screen viewing' in the title.

      In line with our response to Point 8 by Reviewer 1 ‘Screen viewing’ has been changed to ‘Screen time’ throughout the manuscript.

   b) It is evident that the authors are comparing their results with other studies conducted in North America, Australia and England yet the title does not show that the study was a comparative one.

   We feel that the international comparisons are not the main focus of the paper but are an important part of it. We have added reference to the international element at the end of the title so it now reads: ‘Physical activity, screen viewing and obesity status in a nationally representative sample of Maltese youth with international comparisons’. If the editor feels that this addition is not necessary we would be happy to remove.

   c) There is data on the Physical activity, screen time obesity status among school aged children in Africa. Why did the authors fail to highlight some of the results from Africa? There could be some lessons to learn by doing so.

      It would be very interesting to compare results to studies in Africa, however we did not find any studies that met our selection criteria which are now outlined more fully in the methods section (Ls 140-142). The HBSC study which we used to compare screen-time includes all European countries, USA and Canada, and unfortunately African countries do not participate in this study.

   d) Can the authors elucidate why Malta has such a high prevalence of obesity? Could it be nature, nurture or both? This needs to be addressed.

      Indeed this is a key question that we would all like to solve. However it cannot be answered in cross sectional studies such as this. The first step towards the identification of causal pathways is to establish, using robust measures, the degree of association between variables. This study is the first to use an objective measure of physical activity and measured weight status in an attempt to establish the potential of activity as a contributor to the high level of obesity in Maltese children.
Reviewer 3:

*In this study the authors examines the Physical activity, screen viewing and obesity status in a nationally representative sample of Maltese youth. The study as 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) **A.** 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.

Sentences have been added to the Background explaining the links between sedentary time and obesity and the importance of studying screen time (Ls 67-72).

**B.** 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 a discussion.

We found this comment a little difficult to decipher. There may be two points here. The first seems to suggest that studies using self-reported screen time and TV viewing show very low associations with obesity compared to accelerometry. If this is what was intended then the point is accepted. We have added a clarification on different associations for different types of screen times and for boys and girls in the background (Ls 65-66). We acknowledge that self-reported measures lack validity and this is a limitation that we have included in the limitation section (Ls 319-320).

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.**

This point is well made. However, screen time should not be regarded as the same outcome as inactivity as assessed by accelerometer which incorporates all non active behaviours. We have reviewed our emphasis on each of the methods in the results (Ls 224-226) and how we have interpreted them in discussion (Ls 236-241)
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.

This is something of course that our results have highlighted and we have attempted to further stress this point in the discussion (Ls 236-241). The overall findings are that inactivity is not explanatory of obesity but does provide information that might help intervention design as some obese groups and girls in general do show lower levels of activity (Ls 237-239).

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.

It is important to acknowledge that all estimates of energy expenditure from accelerometers are influenced by the characteristics of the sample within which the equation was developed. Thus given the unique nature of our sample, we do not think it would be appropriate to apply such an equation to this study. We have however highlighted in the limitations that we have assessed the total volume of activity and MVPA and not energy expenditure (Ls 316-319).

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 distributions of the residuals looked like?

A sentence has been added in the method section (Ls 148-149) explaining that tests have been done to check normality.

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
We have reorganised the discussion and conclusions section to ensure that findings for each research section have been discussed and summarised.

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).

We have now highlighted the strategy for searching and selecting studies in the methods section (Ls 140-142) covered in our responses to other reviewers.

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.

We have added a sentence in the methods explaining why we have chosen Evenson’s cutpoints (Ls 134-135) and also added it as a limitation (Ls 315-316).

9) 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.

We have highlighted in the limitations (L 313-314) that we cannot describe what children are doing while they are sedentary or active.

10) 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.

Assessing energy balance was not the purpose of this study as this would have been complex and beyond resource. We would have liked to have been able to calculate energy expenditure but as explained in our reply to point 4 above, this was not possible and we recognised this as a limitation (Ls 316-319).
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.

‘assessed’ now changed to ‘computed’ (L33).

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.

‘measure’ changed to ‘assessed’ (L32).

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.

We use weight status as a term to reflect category of BMI as has been the case in several other papers. This is explained in the methodology.

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

Now corrected (L91).

02.04.14