PEER REVIEW HISTORY

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ARTICLE DETAILS

<table>
<thead>
<tr>
<th>TITLE (PROVISIONAL)</th>
<th>Multiple lifestyle behaviours and overweight and obesity among 9-11 year old children: Results from the UK site of the International Study of Childhood Obesity, Lifestyle and the Environment</th>
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<tr>
<td>AUTHORS</td>
<td>Wilkie, Hannah; Standage, Martyn; Gillison, Fiona; Cumming, Sean; Katzmarzyk, Peter</td>
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VERSION 1 - REVIEW

<table>
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<tr>
<th>REVIEWER</th>
<th>Markus Juonala</th>
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<tr>
<td>University</td>
<td>University of Turku, Finland</td>
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<tr>
<td>REVIEW RETURNED</td>
<td>15-Dec-2015</td>
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GENERAL COMMENTS

This paper examines the associations of different lifestyle factors with childhood obesity.

Main comments:
1) What is the reproducibility of ActiGraph measurements?
2) Are results similar in both genders? If not, they should be presented separately?
3) Are there any age interactions?

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<td>University</td>
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GENERAL COMMENTS

This is a well written paper describing the independent and interactive associations between a number of lifestyle behaviours and the prevalence of overweight and obesity in the UK sample from the ISCOLE study. The study is clearly outlined and utilises sound methodology. The higher impact papers from ISCOLE will come from pooling/cross comparison of international studies/sites that adopted a consistent testing methodology. However, I still believe that results from this site specific study holds merit but may be improved after consideration of the comments below.

General comment 1: US and UK spelling is used interchangeably throughout: (e.g. page 3, line 33 ‘standardized’, page 2, line 14 ‘analyse’)

General Comment 2: The general assumption that accelerometers provide a good objective measure of sleep should be toned down in the paper. Although, a reference is provided, I would recommend providing more detail on how ‘sleep duration’ (see specific comment 5) was obtained for this specific paper and I would suggest
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behaviour are used interchangeably. I would recommend referring to 
the measure/behaviour as TV+video/computer use or consistently 
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limitations section which screens were assessed. Given the date 
(2011) of data collection the children may well have been using 
varied types of screens (e.g. tablets, smart phones, etc.) not 
captured in this paper. 
General Comment 4: In the introduction the authors talk about the 
benefits of ‘physical activity’, which one would assumes could 
include light-vigorous physical activity. However, in the analyses the 
authors chose to only explore MVPA. Thus it might be worth 
referring to specific intensities of physical activity and its association 
with overweight/obesity in the introduction as a rationale for the 
alyses. 
General Comment 5: The rationale for including only MVPA and 
substituting self-reported sedentary time (‘screen use’) for 
accelerometer assessed screen time in the models is quite weak. 
Although I recognise the co-linearity issue, techniques have been 
proposed to combat this issue (e.g. isotemporal substitution, 
compositional analysis, etc.) however, recognising that these 
techniques can make interpretation challenging, I think that there 
needs to be a stronger rationale for why MVPA was chosen and 
whether the authors had explored other intensities in separate 
models. 
General Comment 6: The discussion is lengthy which is partly due to 
the number of lifestyle behaviours covered. It would benefit from 
being cut-down and focusing more on the discussion of the specific 
results and, given the limitations in the methodology, not 
overstretching the interpretation. 
Specific Comment 1: Abstract: Page 2; line 12: Put BMI in between 
‘Organization’ and ‘cut-points’ 
Specific Comment 2: Methods: Page 5: Line 92: ‘West’ should be 
capitalised. 
Specific Comment 3: Methods: Page 7: ‘Self-reported measures’: I 
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used to reduce the accelerometer data. 
Specific Comment 5: Sleep duration is of interest to many in the field 
at present. The authors provide the reference to the algorithm used; 
however, pending word count allowance I would suggest that more 
detail is presented on how the data was managed to obtain an 
indicator of sleep duration. 
Specific comment 6: Page 7: Line 131-132: The authors state: 
“Since TV viewing has been extensively reviewed already and was 
examined in the 
12-country study,[15] we sought to examine the role of overall 
screen time (ST).” 
Although this is one reason, I think that the more weighty advantage 
of looking at overall screen time rather that TV watching is that TV 
watching is only one sedentary behaviour. 
Specific Comment 7, Page 7: Line 139-140: Provide a rationale for 
the choice not to include computer based school work in the home in 
the screen measure. 
Specific Comment 8: For readers not familiar with FFQ it would be 
useful to highlight whether this score (and category ‘healthy’ and
'unhealthy') includes sugar sweetened beverages.
Specific Comment 9: Page 8: Line 165-167: Need to be clear how this was established. Were both parent’s education coded, or in some case just one parent? What happened in the case that data was only available for one parent?
Specific Comment 10: Results: Is it possible to include how many children were invited to take part vs took part (to establish any selection bias)
Specific Comment 11: Results: I would recommend stating, briefly, the percentage that obtained 4, 5, 6 and 7 valid days of accelerometer data.
Specific Comment 12: Page 12: I would suggest presenting results split by gender in Table 1 and including wear minutes and time spent sedentary and in light activity (irrespective of whether these are included in the model). Likewise height and weight should be included.
Specific Comment 13: Page 12: A brief written description of table 1 is required, at the very least orienting the reader to some of the more important variables/findings.
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Specific Comment 16: Page 19: Line 340: In the following sentence: “In contrast, objectively measured sedentary behaviour and other forms of self-reported…” ‘behaviour’ should be replaced with ‘time’. 
Specific Comment 17: Page 21: I would caution against strong comparisons between physical activity and diet in the discussion. Diet is very difficult to assess and likely suffered from self-report bias (which was mentioned in the limitations section). Whereas the measure of MVPA (although not perfect) was objectively measured. 
Specific Comment 18: Page 23: 422: The authors state that they had an objective measure of sleep (as a strength). I don’t believe that an accelerometer can provide an objective measure of sleep more an objective proxy for sleep.
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Specific Comment 20: Although I believe the ‘stringent measurement and quality control procedures’ is a significant strength across sites in the ISCOLE study I don't believe it warrants mention as a strength in site specific paper. One should assume/hope that all published papers should have adopted a high quality measurement protocol.

REVIEWER
Enrique Regidor
Universidad Complutense de Madrid
Spain

REVIEW RETURNED
20-Dec-2015

GENERAL COMMENTS
It is a very well written article and provide some interesting results. However, the main limitation of this study is that it does not offer a clear message. If the results of table 2 (obesity) are observed, the findings are in one way. But if the results of table 3 and figure (BMI z-score) are observed, the results are different. This is due to three
reasons. First, the measurement of the independent variables is different in analyzes carried out for table 2 and on assessments for table 3. Second, the diet has been defined differently in each analysis. And thirdly, in the analysis of table 2 it has not been evaluated the interaction.

In the introduction the authors suggest the importance of assessing the potential interaction between lifestyles in relation to obesity. Therefore, this should be your goal, whether obesity used as the dependent variable, as if the dependent variable is the BMI z-score. The first objective that the authors have proposed is irrelevant to the theoretical framework. For this reason, in the current form, they get contradictory results.

On the other hand, if the authors believe that the measurement of the independent variables should be based on the recommendations of the government, this is how they have to measure these variables about lifestyles. And then present the results of interactions using the same variables, whether the dependent variable is obesity, as if the dependent variable is BMI z-score.

Likewise, the way in which the authors have presented the results in table 3 is incorrect. Model 1 must be included. But model 2 should not be included, since interaction has been detected. And in the results of model 3 the reference category should not appear (ie zero in the independent variables should not be included). Precisely because it has been detected interaction! When there is interaction it is necessary to estimate values of the dependent variable (combination of betas) relative to the reference category. They are four categories, except for the case of six categories in the interaction of diet and physical activity. It is also possible to show the estimates that the authors have calculated in Figure 1. But, from my point of view, it is much clearer to the reader to show the estimates in a table, as I noted earlier. Especially if they also include interactions with the models of obesity (in this case the value of the reference category for the odds ratio would be 1).

Finally, we must remember that this is a cross-sectional analysis and therefore is difficult to guess the reasons or the mechanisms that explain the associations found. Therefore it is difficult to suggest recommendations based on the findings. In the conclusions, the authors point out, based on the findings, the need to take into account the recommendations of physical activity. Then, why the authors do not added that, based on the findings, the ideal is to make physical activity and an unhealthy diet?

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Markus Juonala

This paper examines the associations of different lifestyle factors with childhood obesity.

Main comments:
1) What is the reproducibility of ActiGraph measurements?

- We recently reported estimates of reliability in this sample (see reference below). As described in that study, reliability ranged from ICCs of 0.78 to 0.86, 0.73 to 0.85 and 0.72 to 0.86 for LPA, MVPA
and total activity counts, respectively, and 0.67 to 0.79 for sedentary time. The corresponding values for girls were 0.80–0.88, 0.70–0.89, 0.74–0.86 and 0.64–0.80. Overall, the 7-day approach to wearing the accelerometer produces acceptably reliable estimates of most accelerometer-derived metrics.

The sentence ‘this protocol has been shown to provide acceptably reliable estimates of most accelerometer-derived metrics’ has been added to the manuscript on page 7, line 131-132.

Reference:


2) Are results similar in both genders? If not, they should be presented separately?

- The results for both genders are similar, although not identical. While it makes sense to include the descriptive statistics separately for boys and girls as requested by Reviewer 2 (i.e., as we now do in Table 1), we do not feel comfortable presenting the main analyses separately due to the small and unequal sample sizes of boys and girls. Further, we did test the interactions between the behavioural variables and gender, and no gender interactions were significant. Accordingly, and coupled with the power issue, we chose not to stratify the results. A note documenting our decision has now been provided in the Statistical Analysis section, Page 11 (Line 224-225): 'No significant interactions were found between the behavioural variables and age or sex, thus results are presented for the total sample.' Lastly, we controlled for age, sex, and SES in the main analyses.

3) Are there any age interactions?

- There are no significant age interactions for any of the behavioural variables included in the analyses. This is likely due to the very small age range of the participants included in the analytic sample (i.e., SD = .04 for age).
- As above, we have included on Page 11 (Line 224-225) that there were no age interactions in the Statistical Analysis section.

Reviewer: 2
Lauren Sherar

Please state any competing interests or state 'None declared':
I have collaborated with one of the authors (Cumming) and co-authored two papers with Standage and one with Gillson. However, none are directly related to this paper.

Please leave your comments for the authors below This is a well written paper describing the independent and interactive associations between a number of lifestyle behaviours and the prevalence of overweight and obesity in the UK sample from the ISCOLE study. The study is clearly outlined and utilises sound methodology. The higher impact papers from ISCOLE will come from pooling/cross comparison of international studies/sites that adopted a consistent testing methodology. However, I still believe that results from this site specific study holds merit but may be improved after consideration of the comments below.

General comment 1: US and UK spelling is used interchangeably throughout: (e.g. page 3, line 33 ‘standardized’, page 2, line 14 ‘analyse’)

Please state any competing interests or state 'None declared':
I have collaborated with one of the authors (Cumming) and co-authored two papers with Standage and one with Gillson. However, none are directly related to this paper.
- With the exception of names (i.e. Pennington Biomedical Research Center; US Youth Risk Behavior Surveillance System; World Health Organization), we have carefully proof-read the manuscript to ensure that UK spelling is used throughout ('standardized' has been changed to 'standardised' on page 6, line 113; page 8, line 162; page 9, line 188; 'realized' to 'realised' on page 26, line 548).

General Comment 2: The general assumption that accelerometers provide a good objective measure of sleep should be toned down in the paper. Although, a reference is provided, I would recommend providing more detail on how 'sleep duration' (see specific comment 5) was obtained for this specific paper and I would suggest presenting it as promising indicator or proxy of sleep duration.

- More detail on how sleep duration was estimated has been provided on page 7-8, line 141-146 and we have added that ‘Accelerometry can also be used to provide a proximal indicator of sleep duration’ (page 7, line 141).
- We have also toned down that accelerometers provide a good objective measure of sleep throughout, and have added to the Limitations section that ‘accelerometers can only provide a proxy measure of sleep duration’ (page 24, line 502).

General comment 3: Self-report screen time and sedentary behaviour are used interchangeably. I would recommend referring to the measure/behaviour as TV+video/computer use or consistently refer to it as screen time but clearly stating upfront and in the limitations section which screens were assessed. Given the date (2011) of data collection the children may well have been using varied types of screens (e.g. tablets, smart phones, etc.) not captured in this paper.

- We have changed the wording so that ‘screen time’ is referred to throughout. We have added that screen time encompasses ‘TV viewing and computer use’ from the first mention of screen time in the Methods section on page 8 line 159-160.
- An additional limitation has been included which states: ‘given that only TV viewing and computer use were assessed, the role of other screen-based pursuits should also be explored in future work’ on line 515-516, page 25.

General Comment 4: In the introduction the authors talk about the benefits of ‘physical activity’, which one would assumes could include light-vigorous physical activity. However, in the analyses the authors chose to only explore MVPA. Thus it might be worth referring to specific intensities of physical activity and its association with overweight/obesity in the introduction as a rationale for the analyses.

- We have provided a brief rationale for the use of MVPA over other intensities of physical activity in the Accelerometry section of the Methods. The sentence ‘MVPA was analysed in this study, as this intensity of activity directly aligns with the UK PA guidelines [27] and because MVPA has been inversely associated with adiposity as opposed to light intensity activity previously,[28]’ has been included on page 7 line 133-135.
- We have also specified that MVPA was associated with lower overweight/obesity in the Introduction section when discussing past research (Reference 19; page 4, line 63-64).

General Comment 5: The rationale for including only MVPA and substituting self- reported sedentary time (‘screen use’) for accelerometer assessed screen time in the models is quite weak. Although I recognise the co-linearity issue, techniques have been proposed to combat this issue (e.g. isotemporal substitution, compositional analysis, etc.) however, recognising that these techniques can make interpretation challenging, I think that there needs to be a stronger rationale for why MVPA was chosen and whether the authors had explored other intensities in separate models.

- As per our response to comment 4 provided by Reviewer 2, we have provided a stronger rationale
for using MVPA in the Accelerometry section of the Methods (page 7, line 133-135).
- We have not explored other intensities in separate models in this manuscript as a detailed focus on differing intensities forms the basis to a separate ISCOLE-related paper.
- In addition to the co-linearity issue, we have added the rationale that using self-reported sedentary time also provides increased specificity (i.e. in terms of the actual behaviours being assessed), which cannot be achieved from using accelerometry-derived sedentary time. This additional advantage has been provided on page 8 line 153-157.

General Comment 6: The discussion is lengthy which is partly due to the number of lifestyle behaviours covered. It would benefit from being cut-down and focusing more on the discussion of the specific results and, given the limitations in the methodology, not overstretched the interpretation.

- The structure of the Discussion section has been changed so that the results in relation to the first objective (i.e. relationships with overweight/obesity) have been discussed first, followed by the results of the second objective (i.e. in relation to achieving behavioural guidelines). In doing so, we have cut down the Discussion section, and in particular removed sections where we felt that the interpretation may have been beyond the study design and methodology that were employed.

Specific Comment 1: Abstract: Page 2; line 12: Put BMI in between ‘Organization’ and ‘cut-points’

- The sentence now reads ‘Participants were classified as overweight or obese using the World Health Organization BMI cut-points’.

Specific Comment 2: Methods: Page 5: Line 92: ‘West’ should be capitalised.

- The word ‘West’ is now capitalised (page 6, line 104).

Specific Comment 3: Methods: Page 7: ‘Self-reported measures’: I would be clear that accelerometers provides an indicator of sedentary time not sedentary behaviour.

- We have now changed ‘sedentary behaviour’ to ‘sedentary time’ (page 8, line 152) and ensured consistency with this throughout the revised manuscript.

Specific Comment 4: Methods: Page 6: ‘Accelerometry’. Need to specify the non-wear criteria used and what software package was used to reduce the accelerometer data.

- The non-wear criteria has now been provided on page 7, line 139-140. The software package that was used (i.e. SAS version 9.3) to reduce the accelerometer data has also been provided on page 8, line 149.

Specific Comment 5: Sleep duration is of interest to many in the field at present. The authors provide the reference to the algorithm used; however, pending word count allowance I would suggest that more detail is presented on how the data was managed to obtain an indicator of sleep duration.

- More detail has been provided on the measurement of sleep duration on page 7-8, line 141-146.

Specific comment 6: Page 7: Line 131-132: The authors state:
“Since TV viewing has been extensively reviewed already and was examined in the 12-country study,[15] we sought to examine the role of overall screen time (ST).”
Although this is one reason, I think that the more weighty advantage of looking at overall screen time rather than TV watching is that TV watching is only one sedentary behaviour.
- Consistent with the Reviewer’s comment, we have changed our rationale and written that ‘we sought to examine the role of overall screen time (ST), in terms of both TV viewing and computer use, in order to capture children’s engagement in more than one screen-based behaviour’, page 8, line 159-161.

Specific Comment 7, Page 7: Line 139-140: Provide a rationale for the choice not to include computer based school work in the home in the screen measure.

- We have added a rationale in terms of the fact that ‘leisure time screen use better reflects a lifestyle choice over computer use for school work,’ page 8, line 168-169.

Specific Comment 8: For readers not familiar with FFQ it would be useful to highlight whether this score (and category ‘healthy’ and ‘unhealthy’) includes sugar sweetened beverages.

- Sugar sweetened beverages were included in the ‘unhealthy’ dietary score, thus we have added this information on page 9, line 187 in the list of food group examples that loaded onto the two components. The reader is also referred to the paper by Mikkilä et al. (2015) published in the International Journal of Obesity, a paper in which the FFQ used within the ISCOLE study (and this paper) is presented.

Specific Comment 9: Page 8: Line 165-167: Need to be clear how this was established. Were both parent’s education coded, or in some case just one parent? What happened in the case that data was only available for one parent?

- In all cases, just one parent was coded so for children who had data for both parents, the parent with the ‘highest’ education level was used (i.e., the variable used was ‘highest parental education’). For example, if one parent had achieved a degree, and another parent had not completed school, data from the parent with a degree was coded. Where data were only available for one parent, this was used. We have provided more details to make this clearer on page 10, line 197-199.

Specific Comment 10: Results: Is it possible to include how many children were invited to take part vs took part (to establish any selection bias)

- We have added how many consent forms were distributed, page 12, line 253 to the Results section.
- In terms of assessing selection bias, we compared exposure/outcome measures between included and excluded participants and reported the findings (i.e. there were no significant differences found) on page 12, line 256-258.

Specific Comment 11: Results: I would recommend stating, briefly, the percentage that obtained 4, 5, 6 and 7 valid days of accelerometer data

- This information has now been provided in the Results section on page 12, line 263-265.

Specific Comment 12: Page 12: I would suggest presenting results split by gender in Table 1 and including wear minutes and time spent sedentary and in light activity (irrespective of whether these are included in the model). Likewise height and weight should be included.

- This information has been added to Table 1, page 13 including the descriptives split by gender.

Specific Comment 13: Page 12: A brief written description of table 1 is required, at the very least orienting the reader to some of the more important variables/findings.
Specific Comment 14: Comment 13: I think there needs some comment on the magnitude of the associations between lifestyle behaviour and BMI. For example, with every N minutes increase in MVPA there was an N reduction in BMI.

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Reviewer: 3
Enrique Regidor

It is a very well written article and provide some interesting results. However, the main limitation of this study is that it does not offer a clear message. If the results of table 2 (obesity) are observed, the findings are in one way. But if the results of table 3 and figure (BMI z-score) are observed, the results are different. This is due to three reasons. First, the measurement of the independent variables is different in analyzes carried out for table 2 and on assessments for table 3. Second, the diet has been defined differently in each analysis. And thirdly, in the analysis of table 2 it has not been evaluated the interaction.

In the introduction the authors suggest the importance of assessing the potential interaction between lifestyles in relation to obesity. Therefore, this should be your goal, whether obesity used as the dependent variable, as if the dependent variable is the BMI z-score. The first objective that the authors have proposed is irrelevant to the theoretical framework. For this reason, in the current form, they get contradictory results.

On the other hand, if the authors believe that the measurement of the independent variables should be based on the recommendations of the government, this is how they have to measure these variables about lifestyles. And then present the results of interactions using the same variables, whether the dependent variable is obesity, as if the dependent variable is BMI z-score.

Likewise, the way in which the authors have presented the results in table 3 is incorrect. Model 1 must be included. But model 2 should not be included, since interaction has been detected. And in the results of model 3 the reference category should not appear (i.e zero in the independent variables should not be included). Precisely because it has been detected interaction! When there is interaction it is necessary to estimate values of the dependent variable (combination of betas) relative to the reference category. They are four categories, except for the case of six categories in the interaction of diet and physical activity. It is also possible to show the estimates that the authors have calculated in Figure 1. But, from my point of view, it is much clearer to the reader to show the estimates in a table, as I noted earlier. Especially if they also include interactions with the models of obesity (in this case the value of the reference category for the odds ratio would be 1).

Finally, we must remember that this is a cross-sectional analysis and therefore is difficult to guess the reasons or the mechanisms that explain the associations found. Therefore it is difficult to suggest recommendations based on the findings. In the conclusions, the authors point out, based on the findings, the need to take into account the recommendations of physical activity. Then, why the authors do not added that, based on the findings, the ideal is to make physical activity and an unhealthy diet?

Based on Reviewer 3’s comments, we have restructured the presentation of the Introduction section slightly to make the objectives and message of the paper clearer. Indeed, we have been more specific that there are two aims: (i) to present the relationships between multiple lifestyle behaviours and overweight and obesity, similar to Katzmarzyk et al.[15] but for the UK only. As such, the independent variables were treated the same as in the 12-country paper (i.e. as continuous variables); and (ii) to explore how lifestyle behaviours interact to influence BMI z-score. We chose to categorise the lifestyle behaviours for this second objective based on whether children were meeting behavioural guidelines or not. This is because, not only is it easier to present and interpret the results between ‘groups’ (i.e.
categorical variables) when examining interactions, but also because it provides a different and more 'applied' perspective that may be useful and interesting for readers within and outside of academia. As a result of being more focussed in our presentation of the objectives, we have moved the sentence 'it is important to explore how lifestyle behaviours interact to influence markers of health' so that it is now on line 85-86, page 5 alongside the second aim of the paper. We have also removed that this was to aid our understanding of where and how we should try to intervene 'to reduce childhood obesity' in order to avoid the confusion that has been pointed out (i.e. to avoid contradictory results because we have looked at interactions with BMI z-score, not childhood obesity).

In terms of Table 3, we included Model 2 in order to show the reader the model building process that was employed. We have now removed the reference categories (i.e. 'zero') from the Table and included a note to describe what the reference categories are instead. We chose to present a figure of the interactions, as we believe they are easier to interpret but we have included the beta estimates for Model 3 in Table 3 as well to show the size of these effects. As we have not tested for interactions with overweight/obesity, as per the aims which we have described above and in the paper, these have not been included in Table 2.

The structure of the Discussion section has also been changed to reflect the two overarching aims of the work so that the results in relation to the first objective (i.e. relationships with overweight/obesity) have been discussed first, followed by the results of the second objective (i.e. in relation to achieving behavioural guidelines). Consistent with the comments of Reviewers 2 and 3, we have also cut down the Discussion section and removed the text wherein we discuss reasons/mechanisms or provided recommendations that may have been overstretched, given the study design and methodology that were employed. In particular, we have taken out the sentence about meeting the PA recommendations from the Conclusion.

**VERSION 2 – REVIEW**

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<td>The authors have responded well to reviewer comments and should be congratulated on a sound publication. No further comments.</td>
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Multiple lifestyle behaviours and overweight and obesity among children aged 9–11 years: results from the UK site of the International Study of Childhood Obesity, Lifestyle and the Environment

Hannah J Wilkie, Martyn Standage, Fiona B Gillison, Sean P Cumming and Peter T Katzmarzyk

BMJ Open 2016 6:
doi: 10.1136/bmjopen-2015-010677

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