PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Objectively measured physical activity and sedentary time: cross-sectional and prospective associations with adiposity in the Millennium Cohort Study
AUTHORS	Griffiths, Lucy; Sera, Francesco; Cortina Borja, Mario; Law, Catherine; Ness, Andy; Dezateux, Carol

VERSION 1 - REVIEW

REVIEWER	Russell Pate University of South Carolina, USA
REVIEW RETURNED	08-Nov-2015

GENERAL COMMENTS	This study takes advantage of an important dataset, and the study design and methods are appropriate. The manuscript is well organized and clearly written. The conclusions are warranted by the findings. However, the quality of the manuscript would be improved by more extensive discussion of the study's major limitation: physical activity was measured only at baseline. The cross-sectional analyses yield findings that are consistent with those of other studies, but the findings of the longitudinal analyses are much less internally consistent. This is not surprising, because physical activity was not measured during the period between baseline and follow-up. The authors should explain why it makes sense to hypothesize that physical activity at age 7 might predict adiposity at age 11, and they should more extensively discuss possible explanations for their inconsistent findings regarding that relationship.

REVIEWER	Patrick Bergman Department of Sport Sciences Linneaus University Sweden
REVIEW RETURNED	30-Dec-2015

GENERAL COMMENTS	Overall I find the study interesting and a study that potentially could contribute to our understanding of the association between physical activity and obesity in children. The study have many strengths both in study design as well as in data handling and measurements used. There are however a number of concers that I think need to be addressed. Comments and thoughts in order of appearance The authors make several claims that increasing physical activity or reducing sedentary time is a key contributor to obesity; “The notion that insufficient physical activity is a key contributor to obesity is

	common, and is supported by the logic of the energy-balance equation." And "The majority of observational studies that have evaluated cross-sectional associations between adiposity and objectively measured physical activity in children suggest that higher activity levels are associated with lower levels of adiposity. " My concern is that even though this may be true based on the information available from cross sectional studies the causality may be questioned. Information from experimental studies, with a better possibility to deduce causal relationships, is much less clear (1). In fact there is doubts whether its possible to change the long term physical activity behavior in children in a meaningful way (2-3). With that in mind I would like to have some information regarding the evidence on obesity prevention/weight loss etc from experimental studies in the background. (One thing that is interesting is the fact that at the same time as people claim that physical activity levels are declining and sedentary behavior is increasing, the level of obesity is stabilising. This would, on an ecological level, indicate that physical activity is not related to the development of obesity.) Another aspect that I think need a bit more explanation and motivation concerns the assessment of physical activity. In the present study physical activity was measured using accelerometry (good thing!) but there are a number of questions that I think needs a better motivation/discussion. Firstly, the authors choose to include those with two valid days or more to the study. This is not sufficient to rank the subjects from least to most active, which is a prerequisite to rank individuals from the least to the most active. Several studies show, quite unanimous, that at least four days of monitoring are needed to be able to with a relatively high confidence (estimated reliability of r~0.7) do so (for example 4). The reason for this is that the short term variability of physical activity is large. The influence of including those with only two days are not discussed. Another aspect is the long term variability of physical activity that is the maintenance of the relative rank or position in a group over time, i.e. tracking of physical activity. Several studies show at best moderate tracking of physical activity over time and that the tracking is lower the longer the observation period is (see for example 5). The consequence of this is that physical activity assessed at an earlier point of time may not be representative of the physical activity in present time. Both these aspects introduce measurement errors and I think that a proper discussion regarding them are needed. Furthermore, it is unclear why physical activity was standardized based on wear time? This implies that physical activity is linearly related to wear time, and I haven't seen any such study indicating that it is. Is it? If it is, why not include those with lower wear times than 10 hours? Another approach would be to simply adjust for wear time in the statistical analysis rather than standardise a priori? Some minor issues regarding the physical activity assessment are: 1) I suggest to use the word "assess" rather than "measure" when talking about physical activity since we are not that exact in our measurements of it. 2) I also suggest that you use the term "average intensity" rather than "total physical activity" since it is the mean count that is calculated and given that the count is used to describe the intensity

	of physical activity it's a better term. 3) When you write for example 100 counts I assume that you mean 100 counts per minute (cpm)? Or is it 100 counts per 15 seconds? Please clarify. On a side note not really related to the study but yet interesting is that you found sedentary time and physical activity to be too related (i.e. multicollinearity) to adjust for one and another in the respective analysis, this have to my knowledge not been reported previously. Do you think that it warrants caution when interpreting studies that have adjusted for them simultaneously? And can they then be said to be separate behaviours? Can this be discussed in any way? I have a bit of trouble reading tables 2 and 3. How was the p-values in table two calculated? And concerning table three I assume that next to every SD you have entered a p-value? But how was it calculated and what does it stand for? What have been compared with what? Please clarify. REFERENCES 1) Hagströmer M, Elmberg K, Mårild S, Sjöström M. Participation in organized weekly physical exercise in obese adolescents reduced daily physical activity. Acta Paediatr. 2009 Feb;98(2):352-4. 2) Wilkin TJ. Can we modulate physical activity in children? No. Int J Obes (Lond). 2011 Oct;35(10):1270-6. doi: 10.1038/ijo.2011.163. Epub 2011 Aug 9. 3) Rowland TW. The biological basis of physical activity. Med Sci Sports Exerc. 1998 Mar;30(3):392-9. 4) Mattocks C, Ness A, Leary S, Tilling K, Blair SN, Shield J, Deere K, Saunders J, Kirkby J, Smith GD, Wells J, Wareham N, Reilly J, Riddoch C. Use of accelerometers in a large field-based study of children: protocols, design issues, and effects on precision. J Phys Act Health. 2008;5 Suppl 1:S98-111. 5) Francis SL, Morrissey JL, Letuchy EM, Levy SM, Janz KF. Ten-year objective physical activity tracking: Iowa Bone Development Study. Med Sci Sports Exerc. 2013 Aug;45(8):1508-14.

REVIEWER	Scott Duncan Auckland University of Technology New Zealand
REVIEW RETURNED	15-Jan-2016

GENERAL COMMENTS	This manuscript describes findings from a large prospective study of physical activity (PA) and adiposity in UK children. In general the manuscript is well written and easy to follow. There are several major strengths of the study, including the very large and diverse sample size, the objective measure of PA and sedentary behaviour, the use of bioelectrical impedance analysis (BIA) to estimate fat- and fat-free mass, and the collection of adiposity data at two time points 2-years apart. Weaknesses of the study include the absence of a PA measure at the second time point, the 9-month delay between adiposity and PA measures at time point 1 (not mentioned in the Discussion), and the lack of dietary intake assessment. The research area is clearly important for a range of public health practitioners and researchers, and the findings will be of interest to a wide international audience.

I have listed some specific comments below the authors may wish to address.

INTRODUCTION
P4, L5-9: The first sentence references studies of trends in English/UK children, but the implication is that obesity has stabilised and that the onset has become younger worldwide. I suggest adding more references or changing the sentence such that it refers only to UK children.

P4, L25-29: Public health authorities do not only promote PA to potentially reduce obesity. There are many other benefits.

P4, L30-32: Similar to the first sentence, it is unclear which 'adults, adolescents, and children' the authors are referring to. Is this worldwide? UK only? The references are a mixture, but I suggest being a little clearer about which populations are being referred to in this entire section.

METHODS
P6, L56-58: Could the authors please provide a reference to justify the non-wear time criteria?

P7, L46-56: The description of the methods for BIA is overly brief and should be expanded given the importance of the accuracy of these estimates for this study. Here are some examples of questions that could be answered. Is this is a hand-to-foot or foot-to-foot device? Were electrodes used – and if so, what areas were used and how was the skin prepared? Standing or supine? Any instructions given to the participants (e.g. Urination beforehand? Lying supine for a given amount of time?)? How was height and wieght meaured (what devices and by whom)? Was there any training of researchers? What equations were used to estimate fat mass and and fat-free mass? Was the criterion validity of these equations established using the same device and in the same populations? The latter question is vital as proprietary equations that are not validated against a gold standard measure of adiposity have the potential to be inaccurate and unreliable.

P9, L145-24: Why is dietary intake not included on this list?

RESULTS
Tables 4 and 5: The change in exposure measures (e.g., 150 cpm) are not explained clearly in the methods and seem rather unusual. From a reader's perspective, it would be better to see changes that reflect actual guidelines or otherwise meaningful or practical differences. Sixty minutes of MVPA or sedentary behaviour perhaps? Granted cpm is more difficult, but perhaps the authors would consider making these changes more intuitive?

DISCUSSION
General comment: I am unsure just how confident the authors can be in the conclusion that PA causes obesity (in this case higher adiposity) from the prospective results, given that a major influence on obesity (diet) was not assessed. For example, perhaps children at time point 1 who ate poorly became obese, and that, in turn, reduced their PA. Those who were inactive at time point 1 were more likely to stay obese at time point 2, but this does not necessarily mean that low PA at time point 1 caused obesity at time point 2. Perhaps the authors could provide a counterargument to this

	point? Related to this comment, I also think the limitation of having no dietary assessment could be covered in more detail in the discussion (such as what this could mean for the final conclusions). P26, L6: Perhaps the authors could comment on the 9-month delay between adiposity and PA measures at time point 1. What potential implications does this have for the interpretation of the cross-sectional analyses?

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Russell Pate
Institution and Country: University of South Carolina, USA Please state any competing interests or state 'None declared': None declared

This study takes advantage of an important dataset, and the study design and methods are appropriate. The manuscript is well organized and clearly written. The conclusions are warranted by the findings. However, the quality of the manuscript would be improved by more extensive discussion of the study's major limitation: physical activity was measured only at baseline. The cross-sectional analyses yield findings that are consistent with those of other studies, but the findings of the longitudinal analyses are much less internally consistent. This is not surprising, because physical activity was not measured during the period between baseline and follow-up.
Response: We thank this reviewer for his positive comments and helpful suggestions for improvement. We recognise this as a limitation to our work and have now acknowledged and discussed this within the paper.

The authors should explain why it makes sense to hypothesize that physical activity at age 7 might predict adiposity at age 11, and they should more extensively discuss possible explanations for their inconsistent findings regarding that relationship.
Response: Thank you for these suggestions; we have revised the introduction by including a brief description of possible links and have extended the discussion to discuss the inconsistency of findings.

Reviewer: 2
Reviewer Name: Patrick Bergman
Institution and Country: Department of Sport Sciences, Linneaus University, Sweden Please state any competing interests or state 'None declared': None declared

Overall I find the study interesting and a study that potentially could contribute to our understanding of the association between physical activity and obesity in children. The study have many strengths both in study design as well as in data handling and measurements used. There are however a number of concerns that I think need to be addressed.

Comments and thoughts in order of appearance

The authors make several claims that increasing physical activity or reducing sedentary time is a key contributor to obesity; "The notion that insufficient physical activity is a key contributor to obesity is common, and is supported by the logic of the energy-balance equation." And "The majority of observational studies that have evaluated cross-sectional associations between adiposity and objectively measured physical activity in children suggest that higher activity levels are associated with lower levels of adiposity. " My concern is that even though this may be true based on the information available from cross sectional studies the causality may be questioned. Information from

experimental studies, with a better possibility to deduce causal relationships, is much less clear (1). In fact there is doubts whether its possible to change the long term physical activity behavior in children in a meaningful way (2-3). With that in mind I would like to have some information regarding the evidence on obesity prevention/weight loss etc from experimental studies in the background.
Response: We thank this reviewer for their detailed and thoughtful review and constructive suggestions as to areas for clarification and improvement of our manuscript. In relation to this first point, we recognise that the literature is inconsistent in this respect and have revised the introduction to clarify this. We have also added references to two reviews showing no or modest effects of obesity prevention programs as well as to an interventional study that exemplifies the difficulties in changing activity habits in children and assigning causal relations between physical activity and obesity based on such study designs.

(One thing that is interesting is the fact that at the same time as people claim that physical activity levels are declining and sedentary behavior is increasing, the level of obesity is stabilising. This would, on an ecological level, indicate that physical activity is not related to the development of obesity.)

Another aspect that I think need a bit more explanation and motivation concerns the assessment of physical activity. In the present study physical activity was measured using accelerometry (good thing!) but there are a number of questions that I think needs a better motivation/discussion.
Firstly, the authors choose to include those with two valid days or more to the study. This is not sufficient to rank the subjects from least to most active, which is a prerequisite to rank individuals from the least to the most active. Several studies show, quite unanimous, that at least four days of monitoring are needed to be able to with a relatively high confidence (estimated reliability of r~0.7) do so (for example 4). The reason for this is that the short term variability of physical activity is large. The influence of including those with only two days are not discussed.
Response: Thank you for this observation. The rationale for including only those with two valid days or more draws on related methodological work from our group cited as follows: Rich et al (2013) Quality control methods in accelerometer data processing: defining minimum wear time. PLoS One; 8(6). In this large-scale study we demonstrated high reliability (r=0.86) when children with ≥ two days lasting ≥10 hours/day were included in analyses; this reliability study also used the same activity measurement device and protocol. We have now cited this study and the value of the reliability coefficient reported there.

Another aspect is the long term variability of physical activity that is the maintenance of the relative rank or position in a group over time, i.e. tracking of physical activity. Several studies show at best moderate tracking of physical activity over time and that the tracking is lower the longer the observation period is (see for example 5). The consequence of this is that physical activity assessed at an earlier point of time may not be representative of the physical activity in present time. Both these aspects introduce measurement errors and I think that a proper discussion regarding them are needed.
Response: Thank you for this observation. We recognise that a single assessment of physical activity and sedentary time is a limitation of our study, and that as a result we are unable to assess tracking of physical activity. This has now been acknowledged more clearly in the discussion.

Furthermore, it is unclear why physical activity was standardized based on wear time? This implies that physical activity is linearly related to wear time, and I haven't seen any such study indicating that it is. Is it? If it is, why not include those with lower wear times than 10 hours? Another approach would be to simply adjust for wear time in the statistical analysis rather than standardise a priori?
Response: Some summary measures, like daily average minutes spent in moderate and vigorous (MV) or sedentary time (ST), have a cumulative dimension as they are related to the observed or wear time: in this instance more observed minutes could correspond to either higher MVPA or ST.In

our sample there was subject variability in average daily observed or wear times. As a consequence, subjects with higher observed or wear times had higher levels of MVPA or ST. Hence observed or wear time could be a confounder. As this reviewer suggests, one statistical approach is to include the wear time in the model to adjust for confounding. An alternative approach is to standardise by wear time to adjust for confounding. We chose to use this latter approach and to derive standardised measures to characterise the population under study and give summary measures (mean and variability) to describe the population (Tables 2 and 3). Similar results were obtained in evaluating the cross-sectional or longitudinal associations showed in Tables 4 and 5. In our reliability study (Rich et al 2013; Plos One) we demonstrated that measures derived from wear time of 10 hours or more had a good reliability and that use of measures from wear time of less than 10 hours might bias toward the null hypothesis.
Our approach has now been clarified in the text and we have referenced our data technical report and reliability study.

Some minor issues regarding the physical activity assessment are:
1) I suggest to use the word “assess” rather than “measure” when talking about physical activity since we are not that exact in our measurements of it.
Response: Thank you for this suggestion; this change has been made.
2) I also suggest that you use the term “average intensity” rather than “total physical activity” since it is the mean count that is calculated and given that the count is used to describe the intensity of physical activity it’s a better term.
Response: Thank you for this suggestion. Although “average intensity” does describe one dimenstion of mean number of counts, we would prefer to retain the term “total PA” to describe this dimension of activity. This is the term most frequently used by other authors publishing in this field (see, for example: Cooper et al. Objectively measured physical activity and sedentary time in youth: the International children's accelerometry database (ICAD). Int J Behav Nutr Phys Act. 2015) although we recognise there is currently no international consensus on terminology.
3) When you write for example 100 counts I assume that you mean 100 counts per minute (cpm)? Or is it 100 counts per 15 seconds? Please clarify.
Response: Thank you for asking us to clarify this: yes, this refers to 100 counts per minute as now more clearly defined in the text and table footnote.

On a side note not really related to the study but yet interesting is that you found sedentary time and physical activity to be too related (i.e. multicollinearity) to adjust for one and another in the respective analysis, this have to my knowledge not been reported previously. Do you think that it warrants caution when interpreting studies that have adjusted for them simultaneously? And can they then be said to be separate behaviours? Can this be discussed in any way?
Response: This is an interesting topic, recently reviewed in “Page et al (2015) Adjustment for physical activity in studies of sedentary behaviour. Emerging Themes in Epidemiology 12.10”. From a causal point of view, adjusting for physical activity (PA) when evaluating the effect of sedentary behaviour (SB) on an outcome suggests that PA confounds their association and is causally related to both SB and the outcome of interest(please see Figure 1 of Page et al). The same argument applies when considering SB as a confounder of PA. Furthermore, as in our study, PA and SB (which we have termed sedentary time) were measured at the same time, we do not consider that there is a causal relationship between PA and sedentary time as determined by a temporal relationship between these dimensions. We therefore agree that adjustment for these behaviours simultaneously within studies warrants caution, as you have pointed out.
We have cited the Page et al paper and noted our rationale on page 24.
I have a bit of trouble reading tables 2 and 3. How was the p-values in table two calculated? And concerning table three I assume that next to every SD you have entered a p-value? But how was it calculated and what does it stand for? What have been compared with what? Please clarify.
Response: Yes, correct, they are p-values in Table 3 – we have now labelled this column. Thank you.

The gender and ethnic differences between total physical activity, sedentary time and moderate-to-vigorous physical activity anthropometric and physical activity variables were assessed in General Linear Models adjusting by season of measurement and a weekend/weekday contrast. Gender was parametrised as a dummy variable (reference category girls) and the p-values represent the Wald test p value of the coefficient associated to the gender variable. Ethnicity was parametrised with a series of four dummy variables indicating mixed, south Asian, Black and other ethnicity (reference category white) and the p-values represent the Wald test p-value of the coefficients associated with each dummy variable. We have now inserted notes in the table footnotes to clarify this. Thank you.

REFERENCES

1) Hagströmer M, Elmberg K, Mårild S, Sjöström M. Participation in organized weekly physical exercise in obese adolescents reduced daily physical activity. Acta Paediatr. 2009 Feb;98(2):352-4.
2) Wilkin TJ. Can we modulate physical activity in children? No. Int J Obes (Lond). 2011 Oct;35(10):1270-6. doi: 10.1038/ijo.2011.163. Epub 2011 Aug 9.
3) Rowland TW. The biological basis of physical activity. Med Sci Sports Exerc. 1998 Mar;30(3):392-9.
4) Mattocks C, Ness A, Leary S, Tilling K, Blair SN, Shield J, Deere K, Saunders J, Kirkby J, Smith GD, Wells J, Wareham N, Reilly J, Riddoch C. Use of accelerometers in a large field-based study of children: protocols, design issues, and effects on precision. J Phys Act Health. 2008;5 Suppl 1:S98-111.
5) Francis SL, Morrissey JL, Letuchy EM, Levy SM, Janz KF. Ten-year objective physical activity tracking: Iowa Bone Development Study. Med Sci Sports Exerc. 2013 Aug;45(8):1508-14.
Response: Many thanks for these helpful references

Reviewer: 3
Reviewer Name: Scott Duncan
Institution and Country: Auckland University of Technology, New Zealand Please state any competing interests or state 'None declared': None declared

This manuscript describes findings from a large prospective study of physical activity (PA) and adiposity in UK children. In general the manuscript is well written and easy to follow. There are several major strengths of the study, including the very large and diverse sample size, the objective measure of PA and sedentary behaviour, the use of bioelectrical impedance analysis (BIA) to estimate fat- and fat-free mass, and the collection of adiposity data at two time points 2-years apart. Weaknesses of the study include the absence of a PA measure at the second time point, the 9-month delay between adiposity and PA measures at time point 1 (not mentioned in the Discussion), and the lack of dietary intake assessment. The research area is clearly important for a range of public health practitioners and researchers, and the findings will be of interest to a wide international audience.
Response: We thank this reviewer for their detailed and positive review and constructive suggestions as to areas for clarification. On this first point, we have now included reference to these limitations within the discussion.

I have listed some specific comments below the authors may wish to address.

INTRODUCTION
P4, L5-9: The first sentence references studies of trends in English/UK children, but the implication is that obesity has stabilised and that the onset has become younger worldwide. I suggest adding more references or changing the sentence such that it refers only to UK children.
Response: Thank you for this suggestion; we have now included additional references to make this clearer.

P4, L25-29: Public health authorities do not only promote PA to potentially reduce obesity. There are

many other benefits.
Response: Thank you for this suggestion: we have now amended this sentence to clarify that there are a number of reasons why public health authorities promote PA.

P4, L30-32: Similar to the first sentence, it is unclear which 'adults, adolescents, and children' the authors are referring to. Is this worldwide? UK only? The references are a mixture, but I suggest being a little clearer about which populations are being referred to in this entire section.
Response: Thank you: we have now indicated that this is a 'worldwide' problem.

METHODS
P6, L56-58: Could the authors please provide a reference to justify the non-wear time criteria?
Response: Thank you: these criteria have been used in other large-scale accelerometer studies; we have now included a reference to support this.

P7, L46-56: The description of the methods for BIA is overly brief and should be expanded given the importance of the accuracy of these estimates for this study. Here are some examples of questions that could be answered. Is this is a hand-to-foot or foot-to-foot device? Were electrodes used – and if so, what areas were used and how was the skin prepared? Standing or supine? Any instructions given to the participants (e.g. Urination beforehand? Lying supine for a given amount of time?)? How was height and wieght meaured (what devices and by whom)? Was there any training of researchers? What equations were used to estimate fat mass and and fat-free mass? Was the criterion validity of these equations established using the same device and in the same populations? The latter question is vital as proprietary equations that are not validated against a gold standard measure of adiposity have the potential to be inaccurate and unreliable.
Response: Thank you: further details have now been added on the weight, height and body fat measurements. In doing so, we have also discussed the principle of BIA. The manufacturer of the device employed to measure BIA - Tanita - do not publish the equations that they use to calculate body fat, and so we are unable to reference them in this paper.

P9, L145-24: Why is dietary intake not included on this list?
Response: The MCS is a general population cohort and to minimise respondent burden did not include measures of dietary intake. We did acknowledge this as a limitation within the 'study weaknesses' section and recognised that in the absence of dietary information we were unable to explore how this may confound or mediate associations between physical activity and adiposity. No change made.

RESULTS
Tables 4 and 5: The change in exposure measures (e.g., 150 cpm) are not explained clearly in the methods and seem rather unusual. From a reader's perspective, it would be better to see changes that reflect actual guidelines or otherwise meaningful or practical differences. Sixty minutes of MVPA or sedentary behaviour perhaps? Granted cpm is more difficult, but perhaps the authors would consider making these changes more intuitive?
Response: Thank you. These increments represent roughly the standard deviations of the exposure variables. These were used so we could try and compare the effect sizes of the different exposures. We have now inserted a new footnote to tables 4 and 5 explaining this more clearly.

DISCUSSION
General comment: I am unsure just how confident the authors can be in the conclusion that PA causes obesity (in this case higher adiposity) from the prospective results, given that a major influence on obesity (diet) was not assessed. For example, perhaps children at time point 1 who ate poorly became obese, and that, in turn, reduced their PA. Those who were inactive at time point 1 were more likely to stay obese at time point 2, but this does not necessarily mean that low PA at time

point 1 caused obesity at time point 2. Perhaps the authors could provide a counterargument to this point? Related to this comment, I also think the limitation of having no dietary assessment could be covered in more detail in the discussion (such as what this could mean for the final conclusions).
Response: The limitation of no dietary assessment has now been acknowledged more clearly in the discussion. We have now included a hypothesis in the introduction section. Furthermore, within the discussion, we have acknowledged the importance of diet but have explained that we feel that studies have not adequately taken activity (MVPA) into account and that we therefore cannot exclude the importance of physical activity. It is likely that both factors are of course important and that a multifactorial approach to childhood obesity across the life course is needed, as we have mentioned.

P26, L6: Perhaps the authors could comment on the 9-month delay between adiposity and PA measures at time point 1. What potential implications does this have for the interpretation of the cross-sectional analyses?
Response: Thank you: this was as we reported a consequence of the study design whereby the accelerometers were posted after the fieldwork and home visits had been completed. We acknowledge that this is a limitation and have discussed the implications of this within the 'strengths and weaknesses' section.

VERSION 2 – REVIEW

REVIEWER	Russell Pate University of South Carolina USA
REVIEW RETURNED	12-Mar-2016

GENERAL COMMENTS	The authors have responded appropriately to the reviewer comments on the initial submission.

REVIEWER	Patrick Bergman Linneaus University, Department of sport sciences, Sweden
REVIEW RETURNED	23-Feb-2016

GENERAL COMMENTS	The Authors have addressed most of my initial concerns. My remaining concern related to the study design. It is questionable whether it is appropriate to answer the questions asked. Too much of unmeasured confounding could have taken place in-between the measurements. This is however nothing that the authors can change. Finally to increase the understanding of the relative contribution of physical (in)activity to the development of adiposity the overall model fit and also partial r2 could be presented.

REVIEWER	Scott Duncan Auckland University of Technology, New Zealand
REVIEW RETURNED	17-Feb-2016

GENERAL COMMENTS	The authors have addressed my comments well.