PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Cross-sectional associations between high deprivation home and neighbourhood environments and health-related variables amongst Liverpool children.
AUTHORS	Noonan, Robert; Boddy, Lynne; Knowles, Zoe; Fairclough, Stuart

VERSION 1 - REVIEW

REVIEWER	Anna Pearce University College London, UK
REVIEW RETURNED	11-Aug-2015

GENERAL COMMENTS	This study is examining the effects of home and neighbourhood environments on a range of health outcomes, for around 200 primary school children living in deprived areas of Liverpool. The authors carried out the primary data collection themselves, and they appear to have used a good set of, on the whole, validated measures. In general, the paper is well written and the findings of interest . However there are a number of changes, both major and minor, that are I believe are required before it is ready for publication. Major comments Objectives: the reported objectives vary slightly depending on where in the paper they are reported. This sometimes makes the paper hard to follow, for example it is not always clear which analyses and results refer to which objectives. This could be easily rectified through tightening the objectives, and then structuring the abstract, methods, analysis and results according to them. This is addressed in more detail in my minor comments. There are two major limitations of the study (which can't be changed, but should be discussed). Firstly, the relatively small sample size (194) and secondly, the low response rate (76 primary schools were invited to take part and 11 agreed; within those schools 549 children were invited to take part and consent was provided for 217). The authors explicitly draw out associations which were significant for one area deprivation group and not the other. And do not report findings in the tables if they were not significant. Is this appropriate given the sample size? Were power calculations carried out? In terms of the response rate, it would be useful to know roughly how many schools and how many children in this age group there are in Liverpool . How did schools that took part in the study differ from those that were invited but did not take part, and from schools in the Liverpool area more generally? Could this have

	biased the results and in what way? Related to the above, the reporting and discussion of results relies too heavily on what was statistically significant. Please present all results in the tables regardless of significance. See Sterne (BMJ, 2001; 322) and Cummins (Arch Ped. Ad. Med, 2010; 164) for the shortcomings of p-values. Please also avoid drawing conclusions around null results (unless set in the context of a power calculation). The absence of associations may be down to an underpowered sample rather than a true null effect. Minor comments Title: The title could better reflect the study, at the moment it's a bit vague. In addition, please don't use acronyms. I think 'high SES' should read 'high deprivation'? (high SES would mean low deprivation). I recommend referring to Liverpool in the title rather than England, given the location of the study Abstract: Setting: please report the number of primary schools and year of study Main outcome measures: according to the objective, the environmental measures are not outcomes but exposures. Results: Some of the results reported here do not address the objective as reported in the abstract. E.g. the associations between deprivation and health (which refer to Objective 1 as described in the background). Either two objectives could be reported in the abstract, or the superfluous results removed. Please always report effect sizes/coefficients – p values carry little information on their own. Strengths and limitations: Other limitations of the study include the small sample size and the low response rate. Background: It is well referenced and the paragraphs are clearly arranged according to the different characteristics in the study – physical activity, home and neighbourhood environments and area deprivation. However I think two things are lacking: 1. the focus seems to be on physical activity and not any of the other (anthropometric) health outcomes addressed in the study. 2. I think this section would benefit from a final paragraph pulling together the previous paragraphs and identifying where further research is needed. The main objective appears to be examining the effects of neighbourhood characteristics on health, stratified by levels of area deprivation. However the objective in the abstract does not explicitly state that the objective was to examine these associations stratified by deprivation. Furthermore, little attention is paid to this is the background – were the authors expecting the effects of environments on PA, overweight etc. to be bigger in one group than another? Methods Ethics: is there an ethics approval number that can be reported? Socio-economic status: I recommend using the term 'area

	deprivation' rather than socio-economic status. SES is usually thought to be referring to individual-level status. The authors say that two IMD median split categorical variables were created to represent high and high-to-medium deprivation. I didn't really understand this sentence. Were there two variables? Or just one variable with two categories – above and below the sample median? I found it hard to judge how different the high and medium/high deprivation areas were. Please report summary stats for the two groups (e.g. median IMD scores within each group). It would also be useful to know the range of IMD scores at the national level. The following paragraph (beginning "the first author….") seems misplaced here. Physical activity and cardio fitness: Please give more information about these two measures. Are they scores, and if so what are their ranges and distributions? Are mean scores an appropriate representation of the data? Home and neighbourhood environment are situated under the outcomes heading. However in the main analysis (i.e. Table 5, they are exposure variables). Please indicate what the distributions of the composite neighbourhood scores were (e.g. inter-quartile range). Analyses: For what proportion of children were missing data filled in using a mean score? Did the authors repeat the analysis only in those children who had complete scores to see if the pattern of results remained similar? The role of the different variables is not always clear. I think this section would be clearer if it could be structured according to the objectives. For objective 1, the environmental variables are outcomes, but in objective 2 they are exposures. Area deprivation is referred to as an independent variable but in some analyses it is used to stratify the results (i.e. it is considered to be an effect modifier). Please define all acronyms. Results: The results would be easier to follow if they were structured according to the objectives, with headings Would the first paragraph be better placed in the methods section? Please report the final % of children originally contacted to take part. Please report effect sizes/coefficients in the text. P-values carry little information on their own. Some variables are treated differently in some analyses than others, without justification. E.g. BMI: in Table 1 prevalence of overweight is presented, whereas in table mean BMI z-score is given. Similarly composite scores for the home and neighbourhood environment are used in the regression models (Table 5), whereas in Tables 3 and 4

	binary measures for each of the questions making up the composite measures are given. This makes it harder to consider the story that the results are telling us across the tables. Were only univariable regression models estimated? Did the authors consider mutual adjustment for both exposure variables? Or examining the association for all children, adjusting for area deprivation? Discussion: The focus seems to be very much on physical activity. However the anthropometric outcomes are also the result of diet, and diet has environmental influences. Please discuss this too. Or, if the focus on this paper is intended to be around physical activity and its associated outcomes, then state this more explicitly in the title/abstract/background/objectives. One of the major limitations of the study is the small sample size and low response rate. These both need to be addressed in the discussion. Tables: Tables should be standalone. Please provide definitions for all acronyms used, and say what tests were used to estimate p-values. Table 1: it would be useful to see characteristics of the entire sample, as well as broken down by deprivation. In order to meet STROBE guidelines, the sample should also be described according to the exposure variables. This could be done in Table 1, or in Tables 3 and 4. Table 5: Please present all data regardless of significance; don't leave cells blank, or miss out results for exposure variables, where results are not statistically significant.

REVIEWER	Andreea Cetateanu Imperial College, London, UK
REVIEW RETURNED	14-Sep-2015

GENERAL COMMENTS	I agree with the authors that it is important to look more closely at environmental determinants of PA and obesity, especially amongst deprived groups. There have a been an increasing number of papers looking at this in recent years, and as the authors acknowledge, there is a need for employing more standardised methodologies across the board for comparability and consistency purposes. The study has several strenghts and limitations. However, as written, there are a number of limitations that dampen my enthusiasm for what this study can contribute. A few comments for the authors to consider: 1. There seems to be some inconsistency in nomenclature use: The title states 'high SES', however the abstract and the paper make it clear that high and medium deprivation groups are looked at. High SES however means low deprivation (it is indeed stated in the Introduction that lower SES are 'families generally at risk or poorer health', the methodology states that 'lower SES was represented by higher IMD', and the Discussion states that 'findings indicate that low SES children represent an important target group.'). Shouldn't therefore the title read 'low SES' instead of 'high SES'? Similarly, it

	is stated that activity levels are highest among lower SES children- doesn't the literature show that higher SES children achieve higher PA levels? 2. Higher bedroom media availability is discussed in terms of sedentary behaviour only- however it is known that media usage is also linked to exposure to unhealthy food marketing, which in turn can lead to higher unhealthy food intake, which in turn can lead to higher BMI- see studies by Emma Boyland. The authors should at least acknowledge that these two cannot be disentangled. 3. A bit more insight on what has been done so far (other research in this area and gaps in the literature) in the Introduction – please see studies by Cooper et al, Harrison et al… 4. Page 7 – lines 16 to 32 – rarely and sometimes can be inter-changeable and what some kids perceive as 'rarely', other kids can perceive as 'sometimes'- I do not see how these two categories are different- what is 'rarely', and what is 'sometimes'? More specific questions such as once a week, twice a week and so on could have been more suitable. 5. The handling of missing data (e.g., mean imputation) is problematic. A more rigorous approach is needed. The authors should at least give a reference to other studies that have used this method or justify using it. Also, is the data missing at random, not at random? 6. Why is ethnicity not included at al? It is known that it is an important confounder and linked to deprivation status. If the sample is mostly white, this should be stated and discussed at least. 7. I would have liked to have seen a descriptives Table not just with the characteristics of study population (Table 1), but a comprehensive one describing all variables used in the analysis (with mean +/- SD or % population for categorical variables), and maybe a column with missing data for each of these variables. This would give a nice snapshot of what is being analysed. 8. I would have liked to have also see a graph representing trends 9. It is stated on page 16- lines 22 to 28 that 'If compared to children living in high SES areas…'- I am not clear on why only MD and HD was used, why not low deprivation groups included as well? 10. Page 16- line 42- '…may also be related to other factors not examined in this study'.- such as? 11. Page 19- lines 2 to 7- 'MD children who experienced fewer restrictions…'- how do we know this? It is stated in the paper that it is actually HD children who have fewer restrictions than MD children. 12. Some repetitions- for example use of the word 'though' a lot and in close proximity – page 21, lines 17 to 31- though appears 3 times – there are other alternative words that can be used

REVIEWER	Neil Small University of Bradford UK
REVIEW RETURNED	06-May-2015

GENERAL COMMENTS	This is a well written and clearly argued article. It addresses an important subject and offers an original contribution the literature. I am happy to support its publication. I have only minor points to make: The link between neighbourhood and physical activity in childhood is an important one to research. This study focuses on areas of high deprivation in Liverpool and on children aged 9-10. A wide range of validated tools are used. Some of these are problematic, self-

	reported physical activity for example is subject to both social desirability biases (as acknowledged by the authors) and to problems of accurate recall. But in the absence of resources to do reliable measures on each child, actigraphs for example, these measures do give some indication of activity that are helpful. Recruitment was from schools and, given the difficulty of recruiting schools to studies, getting 10 to take part and then 217 children from 549 possible in these schools is good. I can't see any discussion of the data relating to biological maturation (p 6 line 45) and any impact of puberty would be interesting to consider. There is no data on children's ethnicity – given differences in growth rates, obesity cuts offs and patterns of exercise in different ethnic groups it would be interesting to have some indication if this was considered for examination in this study. I thought the point about the impact of heterogeneity of methods and definitions in studies re environmental perceptions and weight status was well made p 17 line 15-20. It was interesting, and encouraging, that Liverpool John Moores University had funded this quite extensive study. The study was approved by the University Research Ethics Committee – it did not go to an NHS Ethics panel. Given that a medical screening form was included in data collection (p5 line 14) I would be interested to see if any view was taken re referring the study for NHS ethics consideration. Detailed points p 4 line 32 – specify what the 90% refers to ie which quintiles of deprivation P18 line 5-10. Given the caveats about data discussed in the article the sentence beginning "In light of these findings.." appears too strong a point to make P22 line 7 – the point is clearly made on p 21 line 39 re the focus on Liverpool – I suggest changing "England" to "Liverpool". Re the STROBE checklist – p36 point 13 re Participants. The detail presented on page 9 of the article does not fully meet this point – can a fuller picture be given?

VERSION 1 – AUTHOR RESPONSE

Response to Comments from Reviewer 1
1. Objectives: the reported objectives vary slightly depending on where in the paper they are reported. This sometimes makes the paper hard to follow, for example it is not always clear which analyses and results refer to which objectives. This could be easily rectified through tightening the objectives, and then structuring the abstract, methods, analysis and results according to them. This is addressed in more detail in my minor comments.
The objectives of the study have been changed so that they are consistent throughout the manuscript (page 1 – lines 4-7, page 4 – lines 5-9, and page 18 lines 2-5).
2. There are two major limitations of the study (which can't be changed, but should be discussed). Firstly, the relatively small sample size (194) and secondly, the low response rate (76 primary schools were invited to take part and 549 children were invited to take part and consent was provided for 217). The authors explicitly draw out associations which were significant for one area deprivation group and not the other. And do not report findings in the tables if they were not significant.
All data regardless of significance level has been presented and there are no longer any blank cells in tables (page 16 and 17).
3. Is this appropriate given the sample size? Were power calculations carried out?
The sample size is similar to other UK based physical activity studies (Eyre et al. 2015; Fairclough and Ridgers, 2010) and was based on access to consenting schools and children, rather than a

power calculation. This, as well as the low response rate are limitations which are acknowledged in the abstract and limitations section of the Discussion (page 2 – line 1, and page 24 – lines 10-11).
4. In terms of the response rate, it would be useful to know roughly how many schools and how many children in this age group there are in Liverpool.
There are 125 primary schools in Liverpool. There is presently no accurate publically available data specifically reporting the number of 9-10 year old Liverpool schoolchildren at the time of the study. The only available population data on Liverpool school aged children relates to children within the age range of 5-16 years (57,371). This figures covers 11 age groups, so assuming enrolment figures were stable over time, there would be around 5200 9-10 year old children.
5. How did schools that took part in the study differ from those that were invited but did not take part and from schools in the Liverpool area more generally? Could this have biased the results and in what way?
Of the ten schools that took part in the study seven schools were located in areas of high deprivation and the other three were situated in areas of medium-to-high deprivation. The schools not included in this study will have been mostly located in areas of high deprivation given that over 90% of Liverpool's population is classified as living in such areas (Public Health England, 2014). Therefore the schools included in this study are likely to have been representative of the population we intended to study.
6. Related to the above, the reporting and discussion of results relies too heavily on what was statistically significant. Please present all results in the tables regardless of significance. See Sterne (BMJ, 2001; 322) and Cummins (Arch Ped. Ad. Med, 2010; 164) for the shortcomings of p-values. Please also avoid drawing conclusions around null results (unless set in the context of a power calculation). The absence of associations may be down to an underpowered sample rather than a true null effect.
All data regardless of significance level has been presented and there are no blank cells in tables.
Minor comments
7. Title: The title could better reflect the study, at the moment it's a bit vague. In addition, please don't use acronyms. I think 'high SES' should read 'high deprivation'? (high SES would mean low deprivation). I recommend referring to Liverpool in the title rather than England, given the location of the study.
The title of the article has been changed to reflect the study design and the participants in the study. The term SES has been replaced with deprivation (page 1 – lines 1-2).
Abstract:
8. Setting: please report the number of primary schools and year of study
Information included (page 1 – line 11).
9. Main outcome measures: according to the objective, the environmental measures are not outcomes but exposures.
For some analyses certain variables may be classed as 'exposure' and for other analyses the same variable could be classified as 'outcome', therefore the term variables has been adopted throughout to avoid any confusion.
10. Results: Some of the results reported here do not address the objective as reported in the abstract. E.g. the associations between deprivation and health (which refer to Objective 1 as described in the background). Either two objectives could be reported in the abstract, or the superfluous results removed.
The objectives have been amended throughout (page 1 – lines 4-7, page 4 – lines 5-9, and page 18 lines 2-5).
11. Please always report effect sizes/coefficients – p values carry little information on their own.
Cohen's d effect sizes reported alongside p-values in results tables and also within the text (page 9 – lines 19-25 and page 10 – lines 1 -3).
12. Strengths and limitations: Other limitations of the study include the small sample size and the low response rate.
These additional limitations have been included in the abstract and limitations section of the discussion (page 2 – line 1, and page 24 – lines 10-11).

Background:
13. The focus seems to be on physical activity and not any of the other (anthropometric) health outcomes addressed in the study.
We have made it clear in the Introduction that body mass index and cardiorespiratory fitness are also important for children's health (page 2 – lines 17-19).
14. I think this section would benefit from a final paragraph pulling together the previous paragraphs and identifying where further research is needed.
An additional sentence stating the rationale for the study has been included at the end of the introduction section (page 4 – lines 3-5).
15. The main objective appears to be examining the effects of neighbourhood characteristics on health, stratified by levels of area deprivation. However the objective in the abstract does not explicitly state that the objective was to examine these associations stratified by deprivation.
The objectives of the study have been changed so that they are consistent throughout the manuscript (page 1 – lines 4-7, page 4 – lines 5-9, and page 18 lines 2-5).
16. Furthermore, little attention is paid to this is the background – were the authors expecting the effects of environments on PA, overweight etc. to be bigger in one group than another?
The positive association between high deprivation and poorer health has been stated more explicitly in the opening paragraph of the manuscript (page 2 – line 24).
Methods
17. Ethics: is there an ethics approval number that can be reported?
Ethical approval number inserted (page 4 – line 23).
18. Socio-economic status: I recommend using the term 'area deprivation' rather than socio-economic status. SES is usually thought to be referring to individual-level status.
The term SES has been replaced with deprivation throughout the manuscript
19. The authors say that two IMD median split categorical variables were created to represent high and high-to-medium deprivation. I didn't really understand this sentence. Were there two variables? Or just one variable with two categories – above and below the sample median? I found it hard to judge how different the high and medium/high deprivation areas were. Please report summary stats for the two groups (e.g. median IMD scores within each group). It would also be useful to know the range of IMD scores at the national level.
Sixty eight percent of the study sample was above the IMD cut-off for the most nationally deprived tertile (26.83). A 50th percentile IMD score of 35.63 was calculated for the sample and one IMD median split categorical variable was created to provide two groups that represent children living in areas of high deprivation (HD) or high-to-medium deprivation (MD). The median and IQR for the HD group was 49.76 (65.2, 40.25) which are all within the national Quartile 1 scores. For the MD group the median and IQR were 22.86 (28.4, 14.0). Both the 22.86 and 28.4 are in the national Quartile 2, while 14.0 is in Quartile 3 [so above the 50% percentile for deprivation]. In light of this the second group were composed of children living in areas of 'high-to-medium' deprivation.
20. The following paragraph (beginning "the first author….") seems misplaced here.
This has been amended and now reads 'the research team' (page 5 – line 13).
21. Physical activity and cardio fitness: Please give more information about these two measures. Are they scores, and if so what are their ranges and distributions? Are mean scores an appropriate representation of the data?
The PAQ-C is a tool that is commonly used in physical activity research. The PAQ-C score ranges from 1 (low activity) to 5 (high activity), and is commonly represented by a mean value (Fairclough et al. 2011; Janz et al. 2008; Thomas and Upton, 2014). According to the PAQ-C scoring protocol, mean score is the summary outcome measure (Kowalski et al. 2004). Cardiorespiratory fitness is also a common measure. The CRF score is based on the number of completed shuttles in the 20mSRT, and total number of shuttles is commonly reported (Boddy et al. 2012; Sandercock, Voss and Gladwell, 2008).
22. Home and neighbourhood environment are situated under the outcomes heading. However in the main analysis (i.e. Table 5, they are exposure variables).

The outcomes heading has been changed to variables (page 5 – line 20).
23. Please indicate what the distributions of the composite neighbourhood scores were (e.g. inter-quartile range).
The means and standard deviations of the neighbourhood variables have been included in table 1 (page 12).
Analyses:
24. For what proportion of children were missing data filled in using a mean score? Did the authors repeat the analysis only in those children who had complete scores to see if the pattern of results remained similar?
The number of children that had data imputed has been included (page 7 – line 25).
25. The role of the different variables is not always clear. I think this section would be clearer if it could be structured according to the objectives. For objective 1, the environmental variables are outcomes, but in objective 2 they are exposures. Area deprivation is referred to as an independent variable but in some analyses it is used to stratify the results (i.e. it is considered to be an effect modifier).
For some analyses certain variables may be classed as 'exposure' and for other analyses the same variable could be classified as 'outcome', therefore the term variables has been adopted throughout to avoid any confusion.
26. Please define all acronyms.
Analysis test names stated in full throughout manuscript.
Results:
27. The results would be easier to follow if they were structured according to the objectives, with headings
The headings objectives 1 and objectives 2 have been included in the results section (page 9 – line 18, and page 10 – line 5).
28. Would the first paragraph be better placed in the methods section?
It is common practice in physical activity related studies to include this information within the first paragraph of the results section (e.g. Carlson et al. 2015; Collings et al. 2014; Fairclough et al. 2012, 2015; Findlay et al. 2010 McMinn et al. 2013), and so for this reason we have left this information where it is..
29. Please report the final % of children originally contacted to take part.
The final proportion of children taking part in the study in relation to those invited has been included (page 9 – line 5).
30. Please report effect sizes/coefficients in the text. P-values carry little information on their own.
Cohen's d effect sizes have been reported in the text and also in table 2, table 3 and table 4 alongside p values (page 9 – lines 19-25 and page 10 – lines 1 -3).

31. Some variables are treated differently in some analyses than others, without justification. E.g. BMI: in Table 1 prevalence of overweight is presented, whereas in table mean BMI z-score is given. Similarly composite scores for the home and neighbourhood environment are used in the regression models (Table 5), whereas in Tables 3 and 4 binary measures for each of the questions making up the composite measures are given. This makes it harder to consider the story that the results are telling us across the tables.
The prevalence of overweight is provided in Table 1 to help describe the population. BMI-Z scores are calculated using age and sex specific reference data, and provide a continuous variable to examine within analyses. Composite scores were the focus of this paper, rather than examining the individual binary measures. Though this analysis would be interesting we felt the composite variables examined the effect of each neighbourhood or home variable.
32. Were only univariable regression models estimated? Did the authors consider mutual adjustment for both exposure variables? Or examining the association for all children, adjusting for area deprivation?
Though this analysis would be interesting, the focus of the paper was to examine differences in variables between the two deprivation groups rather than examining deprivation as a continuous

variable. In keeping with the theme of the paper we felt that examining group (i.e., high deprivation and medium-to-high deprivation) specific associations would align more with the aims of the paper.
Discussion:
33. The focus seems to be very much on physical activity. However the anthropometric outcomes are also the result of diet, and diet has environmental influences. Please discuss this too. Or, if the focus on this paper is intended to be around physical activity and its associated outcomes, then state this more explicitly in the title/abstract/background/objectives.
Some discussion related to food intake has been included in the discussion section of the manuscript (page 18 – lines 22-25).
34. One of the major limitations of the study is the small sample size and low response rate. These both need to be addressed in the discussion.
These additional limitations have been included in the abstract and limitations section of the manuscript (page 2 – line 1, and page 24 – lines 10-11).
Tables:
35. Tables should be standalone. Please provide definitions for all acronyms used, and say what tests were used to estimate p-values.
All analysis information and full definitions have been included (page 8 – lines 10–13).
36. Table 1: it would be useful to see characteristics of the entire sample, as well as broken down by deprivation. In order to meet STROBE guidelines, the sample should also be described according to the exposure variables. This could be done in Table 1, or in Tables 3 and 4.
The means (SD) and percentages of all measured variables in the study have been included in table 1 (page 11 and 12).
37. Table 5: Please present all data regardless of significance; don't leave cells blank, or miss out results for exposure variables, where results are not statistically significant.
All data regardless of significance level has been presented and there are no blank cells in tables.

Response to Comments from Reviewer 2
1. There seems to be some inconsistency in nomenclature use: The title states 'high SES', however the abstract and the paper make it clear that high and medium deprivation groups are looked at. High SES however means low deprivation (it is indeed stated in the Introduction that lower SES are 'families generally at risk or poorer health', the methodology states that 'lower SES was represented by higher IMD', and the Discussion states that 'findings indicate that low SES children represent an important target group.'). Shouldn't therefore the title read 'low SES' instead of 'high SES'? Similarly, it is stated that activity levels are highest among lower SES children- doesn't the literature show that higher SES children achieve higher PA levels?
The manuscript now consistently uses the term deprivation rather than SES.
2. Higher bedroom media availability is discussed in terms of sedentary behaviour only- however it is known that media usage is also linked to exposure to unhealthy food marketing, which in turn can lead to higher unhealthy food intake, which in turn can lead to higher BMI- see studies by Emma Boyland. The authors should at least acknowledge that these two cannot be disentangled.
The suggestion made by reviewer 2 is a valid point and we have included additional information on the association between TV viewing and energy intake (page 18 – lines 22-25).

3. A bit more insight on what has been done so far (other research in this area and gaps in the literature) in the introduction – please see studies by Cooper et al, Harrison et al…
We were unable to locate any relevant information from these two authors suggested by the reviewer, but we have included some additional information on prior research and current gaps in the literature in the Introduction section of the manuscript (page 2 – lines 25-27).
4. Page 7 – lines 16 to 32 – rarely and sometimes can be inter-changeable and what some kids perceive as 'rarely', other kids can perceive as 'sometimes'- I do not see how these two categories are different- what is 'rarely', and what is 'sometimes'? More specific questions such as once a week, twice a week and so on could have been more suitable.

We agree that the options could be interpreted as being somewhat ambiguous but these are the options provided in the validated scale (Salmon et al. 2004). An alternative interpretation (which is the one we adopted) would be that 'rarely' represents an event that doesn't happen very often, while 'sometimes' infers that the event occurs relatively more frequently. Moreover, the scale has been used in other recent child physical activity studies (Atkin et al. 2013; McMinn et al. 2013).
5. The handling of missing data (e.g., mean imputation) is problematic. A more rigorous approach is needed. The authors should at least give a reference to other studies that have used this method or justify using it. Also, is the data missing at random, not at random?
This imputation approach has been used before in previous physical activity studies involving children (e.g., Corder et al. 2010). The children with missing data did not demonstrate any characteristics that suggested there were any systematic patterns to the missing data. On this basis, we believe that the data were missing at random, and thus the missing observations were unlikely to have altered the study's conclusions.
6. Why is ethnicity not included at all? It is known that it is an important confounder and linked to deprivation status. If the sample is mostly white, this should be stated and discussed at least.
It is an excellent point made by the reviewer and we have included data on ethnic background of the participants in the methods section (page 4 – lines 20-21).
7. I would have liked to have seen a descriptives Table not just with the characteristics of study population (Table 1), but a comprehensive one describing all variables used in the analysis (with mean +/- SD or % population for categorical variables), and maybe a column with missing data for each of these variables. This would give a nice snapshot of what is being analysed.
The means (SD) and percentages of all measured variables in the study have been included in table 1 (page 11 and 12).
8. I would have liked to have also see a graph representing trends
In order to avoid replication of data between tables and figures the results have been presented in table format. In addition, the authors are unclear about the trends referred to by the reviewer.
9. It is stated on page 16- lines 22 to 28 that 'If compared to children living in high SES areas…'- I am not clear on why only MD and HD was used, why not low deprivation groups included as well?
It may have been informative to compare against a low deprivation group however, the majority of the city is classed as being deprived. Because 68% of the study sample were above the IMD cut-off for most nationally deprived tertile (26.83), a 50th percentile IMD score of 35.63 was calculated for the sample and two IMD median split categorical variables were created to represent children living in areas of high deprivation (HD) or high-to-medium deprivation (MD).
10. Page 16- line 42- '…may also be related to other factors not examined in this study'.- such as?
Information regarding the influence of diet on weight status has been included to make it clear that physical activity is not the only modifiable factor associated with weight status (page 18 – lines 22-25, and page 24 – lines 19-20).
11. Page 19- lines 2 to 7- 'MD children who experienced fewer restrictions…'- how do we know this?
It is stated in the paper that it is actually HD children who have fewer restrictions than MD children. This statement was more ambiguous than intended and we have adjusted the text to be clearer (page 21 – lines 13-15).
12. Some repetitions- for example use of the word 'though' a lot and in close proximity – page 21, lines 17 to 31- though appears 3 times – there are other alternative words that can be used.
The term though has been replaced to reduce the frequency of its use (page 23 – line 24).

Response to Comments from Reviewer 3
1. I can't see any discussion of the data relating to biological maturation (p 6 line 45) and any impact of puberty would be interesting to consider.
It may have been interesting to examine the effect of biological maturation however there was no interaction effect of maturation. Moreover, this was not the major focus of the study.
2. There is no data on children's ethnicity – given differences in growth rates, obesity cuts offs and patterns of exercise in different ethnic groups it would be interesting to have some indication if this

was considered for examination in this study.
Ethnicity data has been included in methods section (page 4 – lines 20-21).
3. The study was approved by the University Research Ethics Committee – it did not go to an NHS Ethics panel. Given that a medical screening form was included in data collection (p5 line 14) I would be interested to see if any view was taken re referring the study for NHS ethics consideration.
Standard practice is for parents/carers to complete a simple medical screening to ensure that the child participant usually participates in school PE and has no medical condition or illness that might compromise their safety to take part in the study. This information is checked and reviewed prior to testing sessions and is essential for children to take part in the fitness assessments. This approach has been fully approved by the Institutional Ethical Committee on several occasions, in addition other studies conducted by the research team that have been approved by an NHS Ethics panel used the same approach (Boddy et al. 2012; Stratton et al. 2007).
4. Detailed points p 4 line 32 – specify what the 90% refers to i.e. which quintiles of deprivation.
Based on the national quintiles (fifths) of the Index of Multiple Deprivation 2010, over 90% of Liverpool residents are in the three most deprived quintiles, with < 10% in the 2 least deprived quintiles.
5. P18 line 5-10. Given the caveats about data discussed in the article the sentence beginning "In light of these findings.." appears too strong a point to make
The terminology used has been changed to reflect the findings of this study. 'The findings of this study suggest that home environmental factors are potentially more important targets than features of the built environment for future interventions aimed at increasing PA levels in UK children'.
6. P22 line 7 – the point is clearly made on p 21 line 39 re the focus on Liverpool – I suggest changing "England" to "Liverpool".
England changed to Liverpool (page 24 – line 21).
7. Re the STROBE checklist – p36 point 13 re Participants. The detail presented on page 9 of the article does not fully meet this point – can a fuller picture be given?
A more detailed description of the sample has been providing including the ethnic origin of the consenting children as well as the final response rate of children included in the analysis (page 4 – lines 20-21, and page 9 – lines 1-7).

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VERSION 2 – REVIEW

REVIEWER	Anna Pearce UCL, UK
REVIEW RETURNED	30-Oct-2015

GENERAL COMMENTS	This paper is improved and in the majority of cases the authors have addressed each of my concerns in their response; however this information is not always provided in the paper itself (I have tried to highlight examples of this below). Below are some minor details that I think need to be addressed. Most have been structured according to the numbered comments given by the authors in their response. Response to specific comments / response 1. Shouldn't objective 2 read "to assess associations between these perceived home and neighbourhood environments and health related variables *stratified by children who were from hd and md areas*?" 3. The authors have inserted a statement to say that the low response rate may have biased results (Page 24, lines 10-11)– please give more information than this – e.g. in what direction they think the bias could be acting. 4. Sample size - Please provide a summary of this information in the paper, so readers are also aware. 5. Schools in Liverpool- Please also ensure that this information is provided in the paper. 9. Main outcome measures: I don't think this change has made it clearer for the reader. I recommend structuring the analysis section according to the objectives. 11. The authors have now included an effect size 'd' in the tables, with no description of what it represents in the methods or results. In the reviewer response the authors said it was Cohen's 'd' but this is

	not stated in paper. Please provide more information. I was surprised that they didn't just report the difference in means between groups (I think this is a standard output from ANOVAs?). 12. Sample size and response rate: Please give more detail about these limitations. About possible direction of bias. Also worth saying that this is a relatively large sample size for a PA study. 15. Please see comment 1. 16. It is good that the authors have acknowledged that PA tends to be lower in more deprived areas, but this was not my question. Objective 2 is looking at the association between the environment and PA etc, stratified by area deprivation. Something in the introduction highlighting why it is important to stratify by medium and high deprivation areas would be useful. 19. High and medium deprivation: It might be useful to have some of this information in the paper 21. PA and Cardio measures - I don't think this information has been reported in the paper? 24. Missing data: Page 8, lines 41-46: please also say how many children had missing data (i.e. those who were not imputed – at the moment this is given in the results). Please also list item missingness in the text or under the relevant tables. 25. See comment 9. Please state where analyses were stratified by MD/HD, and why. 35. Standalone tables: Table 4 title needs amending as per the others Additional: • Please give a rationale for the adjustment strategy. • There is an OR in Table 3, with no explanation in the methods? • Table 5: Say what type of regression (linear?)

REVIEWER	Professor Neil Small University of Bradford, UK
REVIEW RETURNED	19-Oct-2015

GENERAL COMMENTS	The revisions requested by previous reviewers have all been adequately addressed.

VERSION 2 – AUTHOR RESPONSE

Response to Comments from Reviewer 1
1. Shouldn't objective 2 read "to assess associations between these perceived home and neighbourhood environments and health related variables *stratified by children who were from hd and md areas*?"
Objective two has been changed throughout the manuscript and now includes stratified by deprivation

group.

3. The authors have inserted a statement to say that the low response rate may have biased results (Page 24, lines 10-11) – please give more information than this – e.g. in what direction they think the bias could be acting.
Direction of bias has been included in limitations section of manuscript [page 24, lines 1-2].

4. Sample size - Please provide a summary of this information in the paper, so readers are also aware.
The total number of primary schools in Liverpool has been included [page 5, line 10].

5. Schools in Liverpool- Please also ensure that this information is provided in the paper.
The term ''the city'' has been replaced with Liverpool [page 5, line 11].

9. Main outcome measures: I don't think this change has made it clearer for the reader. I recommend structuring the analysis section according to the objectives.
The analysis section of the manuscript has been restructured according to the study aims [page 9 – line 9 & 16].

11. The authors have now included an effect size 'd' in the tables, with no description of what it represents in the methods or results. In the reviewer response the authors said it was Cohen's 'd' but this is not stated in paper. Please provide more information. I was surprised that they didn't just report the difference in means between groups (I think this is a standard output from ANOVAs?).
The Cohen's d calculations for study aim 1 have been reported in the methods section of the manuscript [page 9, line 15-16].

12. Sample size and response rate: Please give more detail about these limitations. About possible direction of bias. Also worth saying that this is a relatively large sample size for a PA study.
Direction of bias has been included in limitations section of manuscript [page 24 – line 5].

15. Please see comment 1.
Objective two has been changed throughout the manuscript and now includes stratified by deprivation group.

16. It is good that the authors have acknowledged that PA tends to be lower in more deprived areas, but this was not my question. Objective 2 is looking at the association between the environment and PA etc, stratified by area deprivation. Something in the introduction highlighting why it is important to stratify by medium and high deprivation areas would be useful.
A rationale for stratifying the linear regression analyses has been provided in the introduction section of the manuscript [page 5 – line 1].

19. High and medium deprivation: It might be useful to have some of this information in the paper
The authors are unsure what this comment refers to. Information on the High and Medium deprivation classifications is located in the methods section of the manuscript [page 6, line 4-9].

21. PA and Cardio measures - I don't think this information has been reported in the paper?
Additional information has been included on the PAQ-C tool and its scoring [page 6, line 21-24].

24. Missing data: Page 8, lines 41-46: please also say how many children had missing data (i.e. those who were not imputed – at the moment this is given in the results). Please also list item missingness in the text or under the relevant tables.
The number of participants included in the analyses is stated in each of the results tables and the

number of children that had data imputed and the number with complete data has been included in the methods [page 9, line 1] and results section of the manuscript [page 10, line 2], respectively. The number of children that had data imputed was 7. This information is provided in the methods section [page 9, line 1].

25. See comment 9. Please state where analyses were stratified by MD/HD, and why.
Where the analyses has been stratified by MD/HD this is stated in the title of the analysis results table [Table 5] and is also stated in the methods section of the manuscript along with an accompanying explanation for stratifying the analyses by deprivation group [page 9, line 17 & 18]. The purpose of this was to test differences in relationships between variables in the two deprivation groups.

35. Standalone tables: Table 4 title needs amending as per the others
Table 4 has been amended to reflect the other tables in the manuscript.

Additional:
• Please give a rationale for the adjustment strategy.
The covariates used in the ANCOVA (cardiorespiratory fitness, BMI z-score and somatic maturation) and MANCOVA (age) analyses are known to influence the dependent variables (BMI z-score and waist circumference: Galavíz et al. 2012; McGavock et al. 2009; Mitchell et al. 2012; cardiorespiratory fitness: Boddy et al. 2014; physical activity: Baxter-Jones, Eisenmann and Sherar, 2005; Fairclough and Ridgers, 2010; Jürimäe, 2015; home and neighbourhood environment variables: Atkin et al. 2013; Jago et al. 2009; D'Haese et al. 2015; Mitra et al. 2014).

• There is an OR in Table 3, with no explanation in the methods?
An explanation of the Chi square test and OR as a measure of effect has been provided in the methods section [page 9, line 12-13].

• Table 5: Say what type of regression (linear?)
Linear regression has been inserted in the manuscript [page 9, line 20 & 21].

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