

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

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| TITLE (PROVISIONAL) | Can the relationship between ethnicity and obesity-related behaviours among school-aged children be explained by deprivation? A cross sectional study |
| AUTHORS | Park, Min Hae; Falconer, Catherine; Croker, Helen; Kessel, Anthony; Saxena, Sonia; Viner, Russell; Kinra, Sanjay |

VERSION 1 - REVIEW

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| REVIEWER | Conrad, David Public Health England , Cheshire & Merseyside Centre |
| REVIEW RETURNED | 04-Nov-2013 |

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| GENERAL COMMENTS | <p>Overall this is a very relevant and well put together paper which addresses a clearly defined and pertinent research question. As such, it is clearly a useful addition to the evidence base on childhood obesity.</p> <p>The paper has some issues which do need addressing but these should only require minor amendments.</p> <p>Overall a very good paper and useful addition to the evidence base. I have some comments/queries and a couple of issues which need addressing (particularly the last two) which should only require minor amendments:</p> <p>Abstract – Methods – giving a single age group of 5-11 could be misleading as presumably there are no children in the sample who fall between the 4-5 and 10-11 age groups of the NCMP?</p> <p>Introduction – line 83 - "In 2010, the prevalence of obesity among 10-11 year olds in England was 20-29% among Bangladeshi, Pakistani and Black ethnic groups, compared to 16-19% in White British children." Can you give figures for the 4-5 age group as well?</p> <p>Line 100 - "few studies have assessed the relationship with lifestyle behaviours, which may provide insight into the underlying drivers of obesity in these groups". Also line 72 "ours is one of few studies that have examined whether this effect is explained by deprivation" Examples of the 'few studies' should be cited. This point is made again in the Discussion and ref 19 is cited but the point should be referenced where it's first made in the paper.</p> <p>Line 116 – you could add a line here explaining that PCTs are now gone and responsibility for NCMP passed to local authorities from April 2013.</p> <p>Line 121 – "Parents of all children from Islington, Redbridge and West Essex PCTs.." Presumably 'All children' means 'all children in</p> |
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| | <p>Reception and Year 6'? This needs to be clearer in the abstract too.</p> <p>Line 141 – “A healthy eating score was derived as a mean of these sub-scores, with a score of less than 5 indicating unhealthy dietary behaviours.” Could you say a bit more about how the scoring and interpretation was determined – eg was this based on figures from recommended guidelines?</p> <p>Line 151 – IMD – specify which version.</p> <p>Line 153 – Some brief definition/explanation of lower output areas might be handy for non-UK readers eg an indication of size.</p> <p>Line 196 – “At every level of deprivation, obesogenic lifestyle was more common among children from Asian and Black ethnic groups than among White children (Figure 1)” – but the graph appears to show overlapping CIs between the White and Black groups for deprivation quintiles 3-5, so these differences are not statistically significant at every level of deprivation (plus there are no Asian children in quintile 5 so this needs to be acknowledged).</p> <p>Line 236 – “There were also differences between Asian and Black groups, with parents of children from Black ethnic groups reporting more unhealthy behaviours than those of Asian children.” – but weren't these differences only apparent in the unadjusted analysis? The CIs for the adjusted odds ratios for the Asian and Black groups overlap, so again these don't appear to be statistically significant differences.</p> |
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| REVIEWER | McNeill, Geraldine University of Aberdeen, Public Health Nutrition Research Group |
| REVIEW RETURNED | 09-Nov-2013 |

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| GENERAL COMMENTS | <p>Major points</p> <ol style="list-style-type: none"> 1. It is not stated how the six PCTs were selected. Please comment on how representative they are of the UK as a whole, and on whether the ethnic minorities are likely to be representative of ethnic minorities in the UK as a whole. 2. Does the inclusion of PCT as a factor in the analysis influence the results? <p>Minor points</p> <ol style="list-style-type: none"> 3. Line 167 what proportion of participants had incomplete data? 4. Line 196 fig 1 suggests to me that in the least deprived quintile there was no evidence of a difference in obesogenic lifestyle, so 'at every level of deprivation' would not be correct 5. Lines 211-2 please give data on response by ethnic group 6. Line 223 the most recent health Survey for England is not 2007 7. Table 2 row subheadings would be more meaningful if entitled 'Low physical Activity', 'High Sedentary Behaviour' and 'Poor Dietary Behaviour' 8. Table 3: I find it very strange that in the adjusted analyses the boys are more likely to have low physical activity, less likely to have high screen behaviour and less likely to have poor dietary behaviour: all three are in the opposite direction to what I believe other surveys show. Please check these figures and, if they are correct, comment in the text on any differences with other national data. |
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VERSION 1 – AUTHOR RESPONSE

Abstract – Methods – giving a single age group of 5-11 could be misleading as presumably there are no children in the sample who fall between the 4-5 and 10-11 age groups of the NCMP?

We agree that this needs clarification, and the abstract has been amended to describe the age groups as 4-5 and 10-11 years (line 35).

Introduction – line 83 - "In 2010, the prevalence of obesity among 10-11 year olds in England was 20-29% among Bangladeshi, Pakistani and Black ethnic groups, compared to 16-19% in White British children." Can you give figures for the 4-5 age group as well?

The figures for the younger age group have now been included (lines 109-110: among 4-5 year olds these figures were 11-18% and 9-11%, respectively).

Line 100 - "few studies have assessed the relationship with lifestyle behaviours, which may provide insight into the underlying drivers of obesity in these groups". Also line 72 "ours is one of few studies that have examined whether this effect is explained by deprivation"

Examples of the 'few studies' should be cited. This point is made again in the Discussion and ref 19 is cited but the point should be referenced where it's first made in the paper.

This part of the introduction now includes a reference to the systematic review by El-Sayed et al (Ref 21), which highlighted the lack of studies considering the differences in obesity aetiology by ethnicity.

Line 116 – you could add a line here explaining that PCTs are now gone and responsibility for NCMP passed to local authorities from April 2013.

We have included an explanation of the abolition of PCTs and the change in responsibility for the NCMP (lines 142-143).

Line 121 – "Parents of all children from Islington, Redbridge and West Essex PCTs.." Presumably 'All children' means 'all children in Reception and Year 6'? This needs to be clearer in the abstract too. Yes, we have now clarified in the text that 'all children' refers to those in Reception and Year 6 in these PCTs (line 147)

Line 141 – "A healthy eating score was derived as a mean of these sub-scores, with a score of less than 5 indicating unhealthy dietary behaviours." Could you say a bit more about how the scoring and interpretation was determined – eg was this based on figures from recommended guidelines?

Some explanation of how we derived this cut-off is now given in the text (lines 171-173: the cut-off score was equivalent to a child meeting 70% of recommended intake of fruit and vegetables)

Line 151 – IMD – specify which version.

This has been specified in line 182 (IMD 2007 for England)

Line 153 – Some brief definition/explanation of lower output areas might be handy for non-UK readers eg an indication of size.

We have included reference to the number of households per LOA in line 185 (400-1200 households)

Line 196 – "At every level of deprivation, obesogenic lifestyle was more common among children from Asian and Black ethnic groups than among White children (Figure 1)" – but the graph appears to show overlapping CIs between the White and Black groups for deprivation quintiles 3-5, so these differences are not statistically significant at every level of deprivation (plus there are no Asian children in quintile 5 so this needs to be acknowledged).

We agree that this statement is not quite accurate, and have now described the pattern in more detail,

specifying the deprivation quintiles for which patterns are observed (lines 230-235).

Line 236 – “There were also differences between Asian and Black groups, with parents of children from Black ethnic groups reporting more unhealthy behaviours than those of Asian children.” – but weren’t these differences only apparent in the unadjusted analysis? The CIs for the adjusted odds ratios for the Asian and Black groups overlap, so again these don’t appear to be statistically significant differences.

Yes, this statement does not apply to the final adjusted analysis, therefore we have deleted this sentence.

1. It is not stated how the six PCTs were selected. Please comment on how representative they are of the UK as a whole, and on whether the ethnic minorities are likely to be representative of ethnic minorities in the UK as a whole.

We have included some detail on the selection of PCTs in the methods section (lines 150-152). We have also included some discussion of how the demographic composition of our study sample compared with national data (lines 250-254)

2. Does the inclusion of PCT as a factor in the analysis influence the results?

PCT was not included as a variable in our final model in order to avoid potential overadjustment (given the inclusion of other variables which were associated with PCT). We have run analyses which included PCT as a covariate, and there was little effect on the overall results.

Minor points

3. Line 167 what proportion of participants had incomplete data?

The proportions of respondents with missing data for the key variables are now reported in lines 206-207.

4. Line 196 fig 1 suggests to me that in the least deprived quintile there was no evidence of a difference in obesogenic lifestyle, so 'at every klevel of deprivation' would not be correct

We agree, and a similar comment was made by the other reviewer. As noted above, we have now described the observations in more detail, specifying the deprivation quintiles for which patterns are observed (lines 230-235).

5. Lines 211-2 please give data on response by ethnic group

We have now provided information on response rates by ethnic group in lines 209-210.

6. Line 223 the most recent health Survey for England is not 2007

We have updated this reference to the Health and Social Care Information Centre’s report on obesity, physical activity and diet 2013 (which includes multiple data sources, including the most recent Health Survey for England); the point made in this section remains the same, with levels of physical activity and consumption of fruit and vegetables for the relevant age groups from national data being similar to those observed in our study.

7. Table 2 row subheadings would be more meaningful if entitled 'Low physical Activity', 'High Sedentary Behaviour' and 'Poor Dietary Behaviour'

We agree, and have amended Table 2 accordingly.

8. Table 3: I find it very strange that in the adjusted analyses the boys are more likely to have low physical activity, less likely to have high screen behaviour and less likely to have poor dietary behaviour: all three are in the opposite direction to what I believe other surveys show. Please check these figures and, if they are correct, comment in the text on any differences with other national data.

We thank the reviewer for drawing our attention to this. The odds ratios for adjusted analyses are not correct – these appear to have been copied incorrectly from an earlier version of the table. The figures have now been corrected, and it can be seen that the associations between sex and lifestyle behaviours from adjusted analyses are in the same direction as those from unadjusted analyses (and in the direction expected based on previous studies).