Author’s response to reviews

Title: Individual socio-demographic factors and perceptions of the environment as determinants of inequalities in adolescent physical and psychological health: the Olympic Regeneration in East London (ORiEL) study

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Author’s response to reviews: see over
Dear Editors,

I enclose the revised manuscript “Individual socio-demographic factors and perceptions of the environment as determinants of inequalities in adolescent physical and psychological health : the Olympic Regeneration in East London (ORiEL) study” in consideration for publication in BMC Public Health.

The two helpful reviews asked for numerous clarifications and compulsory revisions. These have been addressed on a point by point basis and are as follows:

**Reviewer 1:**

Thank you for your helpful, detailed and encouraging comments which have been taken into account. The following minor essential revisions have been made:

1. The authors should take care in their language used. “Pathways” are mentioned in both the abstract and body of the text, but this study does not specifically look at pathways, since it is cross-sectional and not longitudinal. I think some caution would be warranted here.

Agreed; references to “pathways” have been removed as appropriate. The social and environmental determinants, in the context of this cross sectional study, are now referred to less deterministically as “factors”, or “elements” that are associated with outcomes.

2. A. The response rate is impressive but insufficient detail is given about the sampling strategy. Were all children in a given year group (within the selected schools) invited to participate, or were students sampled? If the latter, how were they sampled?

There was enormous variation in the size of the 7th year form entry, ranging between ~100 to 320 pupils across all schools. In order to achieve the required sample the whole school year was sampled for seven schools. 18 other schools had a subset chosen by a teacher according to timetable arrangements. Therefore no direct selection of students was involved based upon abilities. In the overwhelming majority of schools the questionnaire took place in PE lessons meaning that students were not streamed by any sort of ability during these sessions (PE isn’t streamed).

The following text has been added which describes the above procedure and offers detail requested by the other reviewer:

To attain sample power the whole school year was surveyed in seven schools. The remaining 18 schools provided an allocation of adolescents selected on the basis of school timetabling logistics. Adolescents were not streamed by academic abilities as the survey was carried out during PE lessons. The most common reason for schools refusal was “research fatigue” with teachers being repeatedly approached by external agencies to participate in research projects. This suggests that random personal preferences of the organising staff were a cause of refusal rather than pupil characteristics.

B. Given the sampling strategy and non-response why did the authors not derive and apply sampling weights to their estimates of prevalence and risk?

Derivation of sampling weights was planned and was attempted but proved impossible. This was due to a lack of information on the denominator population. Accurate information on the
number of pupils present on a given day of survey was unavailable as was an accurate count of the total number of pupils enrolled at the school on a given day. Indeed, one school could not even provide information on how many form entries they had in year 7. Therefore the response rate provided is a simple estimate of the proportion of students who participated from the sample offered to the field team during the organising process. The numbers of students who did not attend school that day, or who had left the school, were largely unknown. Discovering this information was theoretically possible, but practically very difficult, so would consequently create a threat to school retention by burdening the school and the facilitating teachers with extra work.

C. Were there differences (i.e. biases) between the students in the CC analyses and those who were not? This would be important to know.

In line with point 3A below, tables 1 to 6 have been re-drafted to present the prevalence/mean scores of each outcome in (1.) the total sample available and (2.) the complete case. This satisfies the request for unadjusted odds/means to be de-emphasised while also supplying information as to whether the complete case analytic sample has significantly different properties to the total sample. There is little difference between the estimates of poor health in the total sample and the complete case sample for the overwhelming majority of associations between covariate and outcomes.

Furthermore, although the complete case analysis obviously had lesser statistical power, this reduction did not alter the magnitude of associations between covariates and outcomes, nor did it render significant associations in the total sample non-significant in the complete case sample. Lastly, most of the significant associations in the unadjusted total and complete case analysis persisted after controlling for covariates and clustering at the school level.

D. Could the authors apply multiple imputation techniques to enhance the methodological approach of their paper?

Agreed, and a sophisticated and challenging multilevel multiple imputation research programme based on ORiEL data is underway. It is expected that further analysis will employ this methodology. However, this manuscript’s focus is to provide readers with a cohort profile of the raw and adjusted data. Being imputation free at this stage will not prejudice the later publication of more detailed analyses based upon observations reported here.

E. I am unsure whether the authors used genuine random effects models to inspect the effect of school clustering on outcomes or whether a simpler cluster approach was taken to adjust standard errors in the models for clustering on the school level?

The former supposition is correct. In the absence of design weights, for reasons explained previously, the multilevel model was used to control for the clustering effect at school level. Further investigations of the individual, home, school, borough effects rightly mentioned are of paramount interest to the aims of the ORiEL study - the specific effects of each of these domains will be covered in fuller detail by later publications. As stated earlier in this response, the intention of this paper is to introduce readers in the public health community to the general characteristics of this local sample focussing on the cross sectional prevalence observed for adolescents at this time. Further and more detailed analysis will fully explore the effect of a range of multi-level environments suggested.

Some statements arising from their manuscript are unsound i.e. “During adolescence perceptions of the urban physical environment are as important as the home
environment in explaining patterns of health inequality” – I think unless you have explicitly modelled the causal levels in your analysis (individual, home, school, neighbourhood) such statements are unsafe.

This wording has been made more specific; the text now refers to the socioeconomic characteristics of the household rather than allude to home as a latent spatial/environmental variable. In line with a previous suggestion, this also reduces emphasis on the relative importance of socioeconomic and environmental factors which cannot be adequately tested using this data.

3. A. While the presentation of univariable i.e. unadjusted prevalence estimates of health outcomes are somewhat interesting for the purposes of descriptive epidemiology, this leads to the presentation of several univariable tables, of which many of the results are not interesting, since the reader would really wish to know whether confounding by other variables influenced any significant associations. I recommend the univariable associations may be shortened and de-emphasised and possibly included only as supplemental data.

Please refer to 2C above.

The tables have been re-drafted to allow the reader to compare more easily the unadjusted with fully adjusted prevalences/means for each outcome. The properties of the complete case sample appear to be representative of the total sample.

B. The authors should present the full multivariable results in Table 7 so that, for example, the association between well-being and gender is already adjusted for depression, physical health and so on. Reading Table 7 alone makes it impossible to conclude whether the reported associations are simply due to confounding by other health outcomes or behaviours.

The contents of the original Table 7 now form the right hand column of the respective tables 2-7. Tables 2-7 now show unadjusted prevalence and mean scores and adjusted means and odds allowing the reader to see where full adjustment led to a change in the probability of the outcome.

Stepwise addition of covariates is not displayed on the basis that such adjustment led to few changes in the risk of illness/mean scores (Full stepwise models have been constructed and are available on request). Differences between exposure categories were stable - for instance, gradients which were present prior to adjustment persisted following adjustment (e.g. association between neighbourhood aesthetics and well-being); categorical differences between groups also persisted (e.g. ethnic differences in general health).

We have not adjusted for other health outcomes simultaneously. We consider that, for instance, adjusting well-being for depression is likely to represent over-adjustment, as they are highly correlated, and the results of simultaneous adjustment for health outcomes, especially in cross sectional analyses, will be not that meaningful and will be difficult to interpret.

6. Some tempering of your discussion would be welcome, and in particular for the conclusion, which makes unsafe statements regarding the relative importance of home vs neighbourhood and implications for policy.
In accordance with the wishes of the other reviewer the discussion has been tempered. Particularly as this is a cross sectional analysis the issues of “relative importance” has been downplayed and conclusions are more specifically based upon the observations from the cohort.

“Our findings suggest that perceptions of the physical environment, along with the social and economic characteristics of their household, are important factors in explaining patterns of health inequality experienced within this cohort.”

We have retained the line on the provision of safe and accessible environments given these factors are relatively neglected by many studies of this age group which tend to place a greater emphasis on socioeconomic determinants. Measurement of environmental perceptions is the novel component of this analysis hence and we feel it warrants the attention given in the discussion. The cross sectional relationship between environment and health has, however, been explicitly acknowledged within the text.

Other matters on this topic:
A. Line 321 – When you say evidence for socioeconomic inequalities is weak, please clarify whether you mean in general or in your study specifically

The text has been drafted to make it clearer that the inequalities appeared to be “mixed” in our study.

B. Line 331-332: You interpret your findings as evidence that neighbourhood perception is more salient that socioeconomic factors in determining adolescent health outcomes. But could it be reverse causation at play?

This specific possibility has been raised within the discussion section.

It is therefore possible that healthier outcomes may positively influence environmental perceptions rather than better environments leading to better health - further longitudinal examination is required to assess causality.

C. Line 334 – There were surprisingly few ethnic differences. Could chance have explained those observed?

Ethnic differences in health have been well documented within migrants as they are often a specific group of interest, or because the older age groups of first generation migrants have higher rates of morbidity (or mortality). Far less is known about the persistence of such inequalities across (often multiple) generations. Further analysis of this data is underway to attempt to show an intergenerational reduction in inequalities due to a socioeconomic as well as behavioural convergence in characteristics towards the profile of the White reference population. It is possible that this cohort reflects such convergence and the lack of difference is due to the cross generational eradication of health differences.

D. Line 351 – 354: Did you have any data on the onset of puberty? What was the rationale for describing 11-12 year olds as adolescents and not children?

No pubertal data was available due to sensitive nature of questions required and the threat they might pose to response rates. These students were termed adolescents on the basis they had just entered secondary school and they would be referred to as such throughout their time in secondary education. Therefore “adolescent” was used to maintain a consistent label throughout the cohort’s life rather than any biological meaning.
E. Line 356-358: I don’t think the reference to the longitudinal study being a valid means of understanding causal processes given your cross-sectional data is valid until other issues are addressed.

And in accordance with the wishes of the other reviewer this claim has been deleted.

F. Line 368: Please check whether reference 49 specifically included black Caribbean and black African groups in its “Afro-Caribbean” group – normally I thought this term was restricted to the black Caribbean group only, which might have an impact on your sentence about ORiEL advancing knowledge in this regard.

I have re-visited Saxena et al, 2002. The researchers combined the Black Caribbean and Black African group to conduct analysis on 807 “Afro-Caribbean” children. Therefore the ORiEL study does indeed advance knowledge by disaggregating this group to look at Black African and Black Caribbean groups separately.

G. Line 382: Even longitudinal studies cannot “confirm” causality.

“Confirm” has been changed to “assess”

7. Some limitations are addressed, but this section will need revisiting given the comments arising here.

A. The abstract could be shortened by omitting the unadjusted results

There is no word pressure on the abstract so these have been retained for interest.

B. In the abstract you state the survey was anonymous but I have reason to doubt this given the longitudinal nature of the follow up.

The questionnaire was pseudo-anonymised and this has now been stated in the text. No identifying information was contained within the questionnaire. The identifier and the personal details were then stored separately and were accessible to a limited number of key fieldstaff.

C. Line 126: In the preceding section the ORiEL study is described as a longitudinal cohort study, but here it is described as a prospective controlled quasi-experiment. This might need some clarification to avoid confusing the reader.

The reference to the quasi-experiment has been dropped to provide clarity and maintain consistency with the previous paragraph. Further detail on the experimental design is available within the referenced study protocol.

D. *Line 128: Please state when (calendar year) the children were assessed in order to put it in the context of regeneration of East London

“Briefly, the cross-sectional baseline survey presented here comprises 3,105 adolescents in year 7 of secondary school (aged 11-12 years) who completed a paper-based questionnaire during the 6 months (January to July 2012) prior to the start of the London 2012 Olympic and Paralympic Games.”
E. Line 143: Individual and household level socioeconomic demographic characteristics are listed here as outcomes but used as exposures throughout.

Socioeconomic characteristics are primary outcomes in the wider ORiEL study – for this study they are exposures as you correctly point out, thank you. The text has been amended so that they are no longer described as outcomes.

F. Line 151-153: Attitudes to the Olympic Park – I did not see these reported in the results. If this is not part of the present study this section can be omitted.

Exposures and outcomes which were measured by the ORiEL questionnaire but which are not reported here have been omitted as suggested. Further detail on these other assessments is available within the study protocol: http://bmjopen.bmj.com/content/2/4/e001840.full

G. Line 163-164: Do you mean “risk of depressive symptoms” or actually “depressive symptoms”? Could a stronger (i.e. more clinical) cutoff have been applied?

“Risk of” depressive symptoms has been removed to indicate that a score of 8 or more indicates depressive symptoms according to clinically defined cut-offs.

H. Line 169-170: Is 75 hours of total activity or more really high? Assuming that corresponds to total waking time, that implies 13.5 hours of sleep per day, which seems a lot, even for adolescents!

Note that the Y-PAQ asks about activity outside of school hours only. Assuming 8 hours a day of school 5 times a week, and 15hrs activity each weekend day, this would allow around 9hrs sleep each day of the week for someone with 75hrs activity per week.

I. Line 183 & throughout: “White UK” and “White Mixed” are not official 2011 Census categories. Please define these as White British and the specific mixed ethnicities to which “white mixed” refers.

Agreed, this section could have and should have been more clearly explained. This section has been re-drafted to explain more accurately that the census categories were adapted to this young sample to incorporate categories specifically targeting known populations in East London. Mixed White encompassed “White UK plus any other background(s)” and this has been added to the text. “White UK” has been used in previous studies in these same East London schools and was retained for future comparative work.

J. Line 239: Have there been recent influxes of white migrants from Europe (i.e. since 2011). I thought most of this migration was before 2011, meaning this would have been expected and captured in the last UK census

It is possible that the census under-enumerated Eastern European arrivals for logistical reasons (highly mobile/transient population; lack of awareness given brief length of residence; wishing to remain clandestine). East London has been under-enumerated in previous years, especially so in 2001. Therefore our counts may more accurately represent the proportions of these groups living in this geographical area.
An alternative explanation arises from our experiences in the field. Fieldwork clearly revealed that a number of school catchments were Eastern European migrant “hot-spots”, particularly so in one borough, and as such our catchment areas may be atypical of the demographic characteristics of the wider area reported by the census. Consequently Lithuanian was the most likely ethnic group in the ORiEL sample to have been born overseas due to their high numbers and recent migratory history within the catchment areas.

Lastly, at follow up one year later there was a significant enrolment of new entrants from Eastern European countries into the ORiEL study further suggesting that we have sampled a dynamic population and have capture demographic trends not apparent in census data.

K. *Table 2 & Lines 248-249: How were univariable differences tested? Please present the test statistics used.

Further detail has been added to the section “Statistical analysis”. The text now states that differences between means (WEMWBS, Physical and sedentary activity) were derived using an unpaired t-test. The table notes describe the significance levels and the symbols used to denote this. For binary outcomes (depressive symptoms, longstanding illness and general health) logistic regression was used to estimate the odds of having the outcome compared to the reference group. The same significance levels were used as for the t-test and these are described in the notes.
Reviewer 2:

Thank you for your helpful comments which have been taken into account and have improved the overall quality of the manuscript. The following minor essential revisions have been made:

1. Abstract line 63. The term “ethnic specific determinants of health” could be interpreted as referring to the interaction of ethnicity and individual sociodemographic or environmental determinants but I do not think this is what is meant here.

This has been changed to make it clearer that we observed that the determinants of health are unlikely to be distributed evenly across ethnic minority groups and this affects their subsequent health profile.

2. How many schools refused to participate? Do you have information on characteristics of these schools compared to those which did participate?

As described in our protocol paper, 41 out of a potential 48 schools were contacted and 25 were recruited. Despite this we do not feel that there was a bias in recruitment towards any type of school:

1. We enrolled the largest and smallest schools in the 4 boroughs, within single sex schools and across a range of religious denominations.
2. The most common reason for refusal was “research fatigue” with teachers being continually approached by external agencies for multiple research projects. This suggests that personal preferences of staff were a cause of refusal rather than pupil characteristics.
3. Comparison with census data (table 1) shows that this sample was closely aligned to that of the local total population of a similar age. Any school-based clustering effects are accounted for during the multi-level analysis.

The above information has been inserted in the text.

3. Sociodemographic characteristics are listed as primary outcomes (measures line 144), which I think is an error. Self-reported general health and longstanding illness are omitted from this list of outcomes.
4. Methods (line 160) should refer to the nationally representative Health Survey for England.

These have been corrected, thank you.

5. Does unemployment refer to those seeking work, or would it be more appropriate to refer to this as not in paid employment? It is surprising that WEMWBS is higher among those in unemployed households.

Respondents were asked whether their parent/carer “had a job” but as you rightly suggest, there is no means of knowing whether this was through choice. This distinction has been added to the text, but for brevity and to aid table clarity, the unemployed term has remained within the tables.

6. I am not familiar with ALPHA but it would be helpful to know if the short or long version was used, how many items were included, and it seems that the scales used here may be different from those included in the cited 2010 paper. Was a factor analysis used here to check the dimensions?
An adapted form of the ALPHA long form was used. Domains included: ‘Distance to local facilities’; ‘Safety’; ‘Pleasant’; and ‘walking and cycling network’.


The following link describes the high ICC values for each domain when scored as an individual scale using the recommended scoring schemes.

http://www.ijbnpa.org/content/pdf/1479-5868-7-48.pdf

Therefore results presented in this manuscript do not derive an overall ALPHA score as all domains were not used as they were not age-appropriate. Consequently each selected domain is interpreted separately as an individual concept. The selection of age-appropriate standalone domains has been made explicit in the text.

7. I don’t think these baseline findings can be said to confirm that the longitudinal component of the study is likely to be valid (line 357 discussion).

The relevant statement has been removed.

8. The discussion refers to interventions to increase energy expenditure but it seems inappropriate to assert they are unlikely to influence other health outcomes since the current study did not capture EE (line 377).

Agreed, and “EE” emphasis has been removed completely as expenditure is unknown and replaced with “physical activity” as this was the outcome assessed by this study.

Discretionary revisions
1. Background line 87. What is meant by the word “continue” in this sentence?

Removed

2. I found the statistical analysis section a little unclear. I think the 3 stages were i) unadjusted means/prevalences; ii) fully adjusted mixed effects linear and logistic regression which allowed for clustering, iii) relationship between health outcomes.

The “statistical analysis” section has been amended and the 4 stages contained in the re-draft have been emphasised in the text.

3. It wasn’t clear to me why the selection effect of sampling working-age families with young children would result in a higher than expected proportion of families with one or both parents working. The same age is used in the census and the sample.

Agreed, and the text has been removed as the ORIEL sample and the census tranche are similar (as stated above) whereas this section of the manuscript is concerned with possible differences between the two.

4. Would nativity be a more usual term to use to indicate country of birth?
“Nativity” has replaced “Generation”.

5. I found the term “environmental distribution” a bit unclear in the discussion (line 319). I think this refers to perceived environmental determinants rather than geographic distribution.

This has been made clearer:

“This paper aimed to identify the socio-demographic and environmental determinants of a range of physical and mental health outcomes in an inner city school-based population of adolescents aged 11 to 12 years.”

6. My reading of the tables is that there was a clear relationship between WEMWBS and family affluence in the fully adjusted models and the effect size looks similar to the environmental characteristics.

Paragraph two (“Mental health and well-being”) of the results section specifically refers to the significantly higher WEMWBS well-being scores for the most affluent group, but this, along with the unemployed lone parent status, was the exception across all socioeconomic indicators for both mental health outcomes. Conversely, environmental effects were observed for most indicators for both mental health outcomes and this is where the emphasis subsequently lies.

7. Could the authors be clearer about what they are suggesting is a useful focus for future analyses (line 370 discussion)?

Further analyses of these data are underway and an overview is now described in the text. Specifically, what components of the environment are most closely associated with health, with reference to the physical and social fabric of the neighbourhood. Reference is made within the text to the importance of these factors in building area based interventions to alleviate health inequalities.

Future studies may examine the extent to which differences in the physical environment (e.g. green or blue spaces, housing) or the social environment (e.g. crime, social cohesion) may explain differences in health with a view to providing evidence for policies aimed at reducing health inequalities via area based interventions.

8. The strengths and limitations section refers to the cross-cultural validity of the WEMWBS, but what about the other instruments used? Also, do the authors know the size of the adolescent population of this area in private education? If sizeable, this may be worth a mention in the limitation section.

Reference was made to the cross cultural validity of the WEMWBS due to its relatively young age as an assessment of mental health and well-being. The SMFQ for depressive symptoms has been employed cross culturally in a previous study in the same geographical area with findings broadly similar (Khatib et al 2010), ethnic meaning of longstanding illness is known to vary by ethnic group (Nazroo 1997) but these ethnic differences appear stable across various data sources (Smith & Grundy, 2011), and self-rated health is well validated across ethnic groups (Chandola & Jenkinson 2000). We can think of no reason for a cultural difference in the reporting of the frequency or duration of physical or sedentary activity however.

No estimate of the private education population is known and this has been added to the limitations section. Line has been added “However, no estimate is available of the number of
adolescents educated privately so results should be generalised at borough level with caution.”

9. The penultimate line refers to the home environment for the first time. It wasn’t clear to me what this captures – is this the socio-demographic exposures?

The “home environment” has been omitted. This has been presented as follows: Our findings suggest that perceptions of the physical environment, along with the social and economic characteristics of their household, are important factors in explaining patterns of health inequality experienced within this cohort.