Reviewer’s report

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

Version: 3
Date: 3 November 2014
Reviewer: Mai Stafford

Reviewer’s report:

I found this to be a very clearly written paper.

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

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

4. Would nativity be a more usual term to use to indicate country of birth?

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.

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.

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

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.

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?

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.