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: 5 Date: 17 December 2014

Reviewer: James Bowes Kirkbride

Reviewer’s report:

Both reviewers asked for a lot of corrections and you’ve done a great job and revising the paper accordingly. I think the paper is now much stronger as a result, though I have a few points which I would still like to see clarified. I hope these will not be too difficult to address.

Compulsory revisions

1. I remain concerned by your concluding line in both the abstract and conclusion paragraph, that “Health planners and providers should ensure that local neighbourhoods provide a safe and accessible environment for young people.” In lay terms, I agree with the sentiment, but as a concluding sentence it implies this is what your work shows, but I don’t think your data support this conclusion. As you acknowledge the key cross-sectional limitation of this data is that you do not know whether perceptions of the neighbourhood are determinants or consequences of mental health processes. Further, you don’t have evidence to show that improving the safety and accessibility of the environment would alter people’s perceptions of the environment. Finally on this important topic, your detailed characterisation of this adolescent sample’s mental health and correlated factors already provides health planners with a wealth of information. You might not need (I think you don’t) this noble, but unsupported conclusion.

2. You confirmed that your models were full multilevel models (presumably random intercepts) with children nested within schools. This is great. But was there any statistically significant variance in your outcomes attributable to the school level? Regardless of its significance, can you please report this in the tables?

3. Your limitation should be briefly extended to acknowledge some of the limitations I raised in my original review, which were eloquently defended, but which nonetheless remain limitations i.e. lack of sampling weights, correct reporting of multilevel models (see my point 2)

Minor revisions

4. P7, 2nd para, line beginning “The schools recruited were the largest and the smallest…” It’s unclear from how this is written whether this was an inbuilt design feature of your sampling method, or that when you looked at your sample of schools you had good spread or variation on these variables (school size,
religious denomination, sex ratios). I assume the latter – perhaps you could re-write the sentence.

5. P7 2nd para, following sentence after that one in my point 4 – how were the 7 schools selected to be completely sampled, vs the 18 who were partially sampled in the year group under study?

6. P11, first para of results: I am not sure the under-representation of some ethnic groups can be “off-set” by the over-enumeration of others. Off-set implies equilibrium, whereas the data are just not quite representative of certain ethnic groups. I think it’s ok to say that.

7. P14, para under “Discussion” – use of the word mediating – these are not mediators but confounders as treated in your analyses. Suggest change.

8. Same para as my point 7, line beginning “This suggests a more salient role for…” – it is only more salient if perceptions of the environment are not auto-correlated with mental health symptoms. i.e. not just really measuring the same latent factor.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests