Reviewer’s report

Title: Intervention dose estimation in health promotion programmes: a framework and a tool. Application to the PRALIMAP trial.

Version: 1 Date: 9 June 2012

Reviewer: Jonathan Graffy

Reviewer’s report:

Thank you for asking me to review this paper. It describes a framework adopted in evaluating intervention fidelity in a trial of school based interventions to promote diet and physical activity. The paper draws on some literature relating to intervention fidelity, although I believe there is rather more literature available on intervention dose and fidelity than they acknowledge. The paper then describes the framework that they developed and used to evaluate the intervention dose delivered in their trial. The prime reason to do this seems to be to run an analysis of outcome based on intervention dose, rather than just treatment allocation.

Overall, I found the paper quite mechanistic and technical, rather than particularly interesting to read. I think it probably is a useful description of the method that they adopted. They confirm the feasibility of the method, but don't really evaluate its utility.

1. Some aspects of their approach were a bit surprising - for example using a binary measure of quality. They may wish to comment on this.

2. I also thought the supplementary information on the trial was essential to understand the context. The title should not just name their trial as context. Both it and the abstract should include the words diet and physical activity or similar pointers to the context. The title refers to a framework and tool, but these are not clearly signposted in the text.

3. The first paragraph of the methods is very long. The word "declined" in the middle of page 4 is technically correct, but perhaps confusing. Maybe "evaluated" would be better.

4. On page 5, the Intervention periods intervention reference to PRALIMAP was unclear. Were the same pupils studied in both years?

5. On page 6, more information is needed about the non-programme activities reported. An example or two would help.

6. The idea of using a fictitious IRG to calibrate was a bit confusing. Why did they not just use between group variability?

7. P 13 - "Adhesion" Do they mean adherence?
8. P13 has 4 questions in the middle of the page about components. These might have been better introduced at the beginning of the paper.

9. The later part of the discussion dragged on a bit and did not include an adequate evaluation of the approach.

10. Although they put forward an approach to evaluating dose delivered, they don’t really consider whether this is applicable in other settings. They just assert it, but perhaps should consider its utility more critically.

I have indicated in the wording of the review which comments are discretionary and which I would really like to see.

**Level of interest:** An article whose findings are important to those with closely related research interests

**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’