Author's response to reviews

**Title:** The cost-effectiveness of adaptive e-learning devices to promote dietary change: an economic model

**Authors:**

- Alec Miners (alec.miners@lshtm.ac.uk)
- Jody Harris (J.Harris@cgiar.org)
- Lambert Felix (Lambert.Felix@lshtm.ac.uk)
- Elizabeth Murray (elizabeth.murray@pcps.ucl.ac.uk)
- Susan Michie (s.michie@ucl.ac.uk)
- Phil Edwards (Phil.Edwards@lshtm.ac.uk)

**Version:** 3  **Date:** 24 February 2012

**Author's response to reviews:** see over
An economic evaluation of adaptive e-learning devices to promote weight loss via dietary change for people with obesity

Thank you for providing the two sets of referee comments relating to our manuscript. We believe we have been able to address all of the comments, as detailed below. We do hope that you now feel able to accept the manuscript for publication in your journal.

Referee 1
1. We thank the referee for her positive feedback and note that the only requested revision was to nuance our conclusion that e-learning devices are unlikely to be cost-effective. To do this, the reviewer suggested referring to a recently published article on a similar topic, in which different conclusions were drawn (Jacobs et al. 2010). We have done this (see the discussion page 13, second paragraph). Indeed, as per Referee’s 2 comments, we have reviewed a number of articles in this paragraph, and added in the nuance that not all ‘e’ based interventions are the same, meaning that the conclusions we draw should not necessarily be generalised across all devices.

Referee 2
1. Again we thank the referee for her useful comments. We agree that the terminology we used was confusing in places. To remedy this, we have made the following changes to the manuscript. 1) The title has been changed to make it clearer that the aim of the e-learning devices in the context of this evaluation is to alter body mass index but the mechanism for doing this is via promoting dietary change. We have also altered the ‘decision problem’ section (page 6, 2nd paragraph) to more clearly reflect this objective. 2) We no longer refer to the comparator intervention as ‘dietary advice’ alone. Indeed the reviewer was quite correct to note that weight loss interventions are rarely used in isolation and a look at the list of control arms in the underpinning systematic review reveals an array of different interventions, not just dietary advice. For this reason, we now refer to the comparator intervention arm as ‘conventional care’, and indicate that this is likely to vary by setting. We not only feel this is more technically correct from an evaluation perspective, but also that it more accurately reflects the evidence we have used to build the model. Note that while this change is important, the costs (and therefore the results) of the evaluation have
not been changed, since the costs of the control arm were taken from a study in which these broader intervention costs (e.g. exercise, slimming clubs) had already been included.

2. A) The referee was correct to state that few details of the underpinning systematic review had been provided. This was intentional in so much that while it is an important component of the study, the focus of the paper is on the cost-effectiveness conclusions and there is clearly a limit to the amount of text that is acceptable. Since submitting the manuscript to your journal, the systematic review has been published in a HTA monograph (Harris, J. et al 2011). Therefore, rather than add in the details of the review and included trials, the monograph is referenced in the manuscript where appropriate. We have taken this approach as a means of saving space / text, but if you feel if this is inappropriate, we would happily provide more details in the manuscript.

B) (Reviewers point 2 continued). The reviewer makes a number of comments regarding the selection of trials in the systematic review and specifically that they appear to be a very 'mixed bag'. First, the reviewer correctly states that the review included RCTs that in which individuals were sometimes below the age of 50 (50 years of age being the starting age in the economic evaluation). To address this comment, we have run another sensitivity analysis with a much lower starting age of 35 (with no impact on the overall conclusions).

C) (Reviewers point 2 continued). We accept and agree that the trials are very different, particularly in terms of the treatments / practices used in the control arms, and this is noted in the review. However, the meta-analysis I² heterogeneity statistic is reassuringly low indicating that despite these differences, the interventions all had a very similar impact on BMI. We therefore respectfully disagree with the implication of the reviewer's comments that the trial results should not be combined. Indeed, we consider this to be a study strength and to represent good practice to use the totality of evidence when estimating treatment effects in economic evaluations, rather than using selective trials in the absence of observed heterogeneity. Lastly, while we recognise the potential importance of adjusting for differences in BMI between groups at baseline, this is rarely possible in the context of systematic reviews because individual level data would be required; most of the trials included in the review only report mean BMI values at baseline and follow-up, which is insufficient to perform the suggested adjustment.

3) The reviewer questions the appropriateness of using a pooled estimate of treatment effect but comparator intervention costs from a single study that appears to include costs that are broader than our definition of ‘dietary advice’. We hope that clarification of our terminology (specifying conventional care to be the comparator rather than dietary advice) helps to clarify the last issue. In principle we agree with the remark that it is best to use estimates of treatment effect and cost from identical sources. However, in reality this is not practical (it is difficult to pool estimates of mean cost, unlike treatment effects) and we note that the results from the sensitivity analysis clearly indicate the cost-
effectiveness of the e-learning devices is not sensitive to this parameter. With respect, therefore, we do not consider this to be an important issue.

4) We agree that we omitted to compare our results with the related literature. As indicated in the reply to Reviewer 1, we have added in a paragraph in the discussion comparing our results with those reported by four other studies, including those suggested by both Reviewers.

5) Minor comments. We have thoroughly revised the referencing.

6) Minor comments. A number of typos and grammatical issues have been addressed.

Yours sincerely,

Alec Miners, PhD
Lecturer in Health Economics,
Department of Health Services Research and Policy
London School of Hygiene and Tropical Medicine