Reviewer’s report

Title: Cost effectiveness of a computer-delivered intervention to improve HIV medication adherence

Version: 1 Date: 23 October 2012

Reviewer: Michael Horberg

Reviewer’s report:

NOTE: I would view these comments as Major Compulsory Revisions.

1. This is a well-written article about a seemingly important intervention using new but available technology. The problem is that the intervention is not adequately described or referenced. A better article would be an analysis of the intervention itself, or at least reference where the intervention (and computer program) can be fully described, and its efficacy discussed. At the very least, please reference the program itself or maybe put in as an appendix, so that readers can better appreciate the program.

2. Another issue is that I am not convinced that the cost data reflects what would be required for a non-research clinic to develop the program. It is not justified well in the text. I think using the budget from the study is a clever concept, but I am uncertain that it’s accurate. For example, saying the participation incentive of $50 covers lost wages is not necessarily accurate, or even likely. The authors need to provide more rationale/data to support the cost data used.

3. Further, the adherence improvement is based on one month pre-/post-. This is so short term for a long term therapy, that the QALY analysis becomes suspect. It would be far better to have longer term adherence data and determine benefit and cost-effectiveness on that data. This is especially so as the authors assert that an increase in adherence is related to higher costs due to higher medication costs (reasonable), but they don’t take into account the higher costs of care of hospitalizations and emergency care, etc., associated usually with poorer adherence and poorer immune status. If these costs are accounted for, that needs to be made more explicit or if implied in the different costs by immune status that should be noted also.

4. Further, the assumption of only 5% movement/improvement of treatment effect from say <50 cells/µL to the next level is too low. That would imply fairly ineffective therapy, and not consistent with today’s treatments.

5. The discussion section is well-written. The conclusions, though, need work, as I’m not sure that as written they are justified by the data. The cost-savings argument needs revision, especially as some of the assumptions made to fit the model (noted above) are questioned.

To address specifically the questions asked of reviewers from the editorial board:

1. Is the question posed by the authors well defined?—Yes.
2. Are the methods appropriate and well described?—Yes. But see above.

3. Are the data sound?—Some. Exceptions noted above.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?—Yes

5. Are the discussion and conclusions well balanced and adequately supported by the data?—Yes, but some analyses could be revised.

6. Are limitations of the work clearly stated?—Yes.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?—Yes.

8. Do the title and abstract accurately convey what has been found?—Yes.

9. Is the writing acceptable?—Yes.

- Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)
- Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

**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'