Reviewer's report

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

Version: 2 Date: 21 January 2013

Reviewer: K. Rivet Amico

Reviewer's report:

The authors have comprehensively addressed many of the concerns from the reviewers. In doing so, however, this reviewer did experience some discomfort over the amount of text devoted to discussing intervention efficacy and implications of the intervention itself rather than remaining focused on cost effectiveness of an intervention estimated to have X through Y efficacy. Specific areas where text seemed excessive in this are noted below. This is a complex manuscript and the authors have clearly taken on the herculean task of estimating costs and potential benefits through available data and pulling from published literature. The addition of Table 1 is appreciated, however, perhaps the authors could consider adding in monetary value to the factors. Because many of these estimated were identified through extensive literature review on the part of the authors, the research community could greatly benefit from having an easy-to-access table of estimates that they could use in estimating cost effectiveness. Several smaller considerations are noted below.

Abstract:
*Typo: “…but many patients to not achieve…” should be “…but many patients do not achieve…”

*Results: Consider revising or deleting first line- ‘The intervention’s cost effectiveness…’ as this is pretty much expected. If retained, as ‘efficacy’ or ‘effectiveness’ of the intervention as well as a driver of cost effectiveness. Also- here and throughout- it would be good to distinguish between continued increase over time (eg., intervention effects continue to amass over time) vs. durability of intervention effects (eg., intervention effects are retained post intervention).

*Duration of greater than one month are noted in abstract but results largely focus on 4 time scenarios (3, 6, 9 and 12) months- it is not clear where the one month reference to outcomes is coming from.

Background:
*It would be helpful to note geography of work early on. The extent to which interventions are available and what they are depends on location or, at a very gross level, country, and feasibility of internet based scenarios will depend on use if internet in communities (also dependent on country).

*Typo? Page 6 ‘Few clinicians are likely to have the time to spend one hour in
providing…’ Should “in” be deleted? ‘Few clinicians are likely to have the time to spend one hour providing…’

*Consider changing ‘efficacious’ to ‘effective’ in paragraph 2 page 6.

*Here and other places, it seems there is text that promotes or ‘defends’ computer delivered approaches. At the end of page 6 the authors note the savings in clinician time. It does not appear that saving of clinical hours/time is in the formula for savings, which is fine as standard of care is not an hour with a clinician and cost effectiveness was in relation to currently available SOC and not to an alternative clinician-delivered intervention. However, given that it is not really a key part of methods or results, it feels odd to have this text in the introduction. It would be sufficient to say that for a variety of reasons computer delivered ART education and aspects of counseling have multiple advantages that impact evaluations of cost-effectiveness, include X, Y and Z, and leave it at that.

*Page 7- The authors may want to revise first sentence on page- presently it is really long (5 lines) and has two uses of ‘delivered’ in close proximity.

*Paragraph 2, page 7- As noted previously, it is not clear why the authors are going into so much detail on why computer delivered may be better that clinician delivered. This seems outside the scope of the research question presented in this manuscript.

*Page 9 through before final sentence in Background- While the added details on the intervention itself is likely in response to reviewer requests, this reviewer strongly preferred the original introduction’s more streamlined approach. From a reader perspective, the content of cost effectiveness analyses is necessarily dense and adding in full coverage of the intervention and intervention efficacy pulls the reader in too many directions. My preference would be to refer people the manuscript describing the intervention, limit details to those that are critical to assumptions in the current manuscript’s analyses, and note range in effect size used. Obviously, publishing outcomes would resolve much of this.

*Page 11, Table 2 + Figure 2. It is not entirely clear that the combination of Table 2 and Figure 2 is really accomplishing proving added clarity to readers. Having to toggle between the two and with text is concerning. Is there any way to combine the two? The meaning of the numbers in the circles on top of Figure 2 are not immediately apparent. Clearly, this is critical in the presentation of results. It may simply need to remain as is. However, if the authors can identify a way to consolidate it would be less cumbersome for the reader.

*Table 2- None of the effectiveness scenarios influenced movement in upper end (over 500 CD4). The reviewer did not see commentary on this- but it is curious and may be worth noting.

*Page 12- Costs of lost salary and wages to travel to and from intervention site are included as “development” cost. Should that be “implementation” cost? Alternatively, if they were included for development because development
included the need for evaluation, perhaps simply note that costs associated with efficacy evaluation are included as necessarily R&D.

*Costs section- Consider adding headers to this section delineating each area in which costs were estimated or known.

RESULTS

*Page 21- Is it possible to anchor the Internet scenario presented in terms of cost per 1% increase, consistent with presentation of clinician office outcomes.

DISCUSSION

*Page 24- Typo? Should first line read “…its effects in decreasing treatment costs due to improving health status” ?

*Page 25- consider tapering comment regarding no decrease in adherence over time from Amico et al. In that research, a number of interventions evaluated did not have a true follow-up period where the intervention was completely removed and individuals were followed over time. While no difference was found in terms of intervention effects and number of weeks post baseline for intervention, it is somewhat of an overstatement to say that adherence over time would remain unchanged in the treatment condition. Consider revising to ‘…suggested no marked deterioration in intervention effects over time.”

Page 28- the paragraph on uptake of computer interventions and limitations in such delivery strategies seems out of place in the current manuscript.

CONCLUSIONS

Page 29- noting that cost effectiveness depends on effectiveness and reach seems unnecessary.

Page 29- review of pros and cons of computer delivered intervention (Table 8) is interesting but seems beyond the stated scope of the manuscript.

TABLE 1- Is it possible to add costs to these? Also, there are a few “[ref]” in the table.

[see attached for formatted version of review]

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.