Reviewer’s report

Title: A Systematic Review and Meta-analysis in the Effectiveness of Mobile Phone Interventions used to improve Adherence to Antiretroviral Therapy in HIV Infection

Version: 0 Date: 25 Feb 2019

Reviewer: Gitau Mburu

Reviewer's report:

It was a pleasure to assess this systematic review. This is a review of an important area related to ART adherence. It is an already strong and detailed review, but would suggest the following discretionary and minor revisions to strengthen the paper even further:

1. Abstract
   a. It is somewhat odd that this paper - which has evaluated several RCTs on adherence, and found that there may be some, or no effect depending on the content of what is delivered through mobile phone (or what is measured e.g. viral load outcomes as proxy indicator of adherence) - suggests that evaluation of cost effectiveness should be performed using RCTs. At best, this is the time for implementation of proven interventions and gathering of cost data as part of implementation research.

2. Background
   a. I suggest referencing should be improved, for example where authors mention poor adherence can lead to resistance (other places e.g. hawthorne effect, or analysis methodology can be better referenced too)

3. Methods
   a. Best to state at the beginning if this review was conducted according to PRIMA and to attached the required checklist.
   b. Interventions: As far as I can see, the authors were assessing use of mobile phones as a channels for delivering different interventions, rather than interventions themselves.
   c. Outcomes: Line 162/163. It is confusing when authors speak of attrition as an outcome, when the primary and secondary outcomes are about ingestion of ARVs. Of course attrition can
affect verified adherence, and ITT analysis should take care of that, but it does not seem appropriate to include or measure loss to follow up from programs as an outcome.

d. How many authors were contacted? Since the review is completed, this information can be disclosed. When authors mention that no discrepancies were observed, it creates the impression that authors already had the data they needed, so it is not clear what additional data were acquired from contacting authors. This could be made clear.

e. The inclusion of search strategy and search text words / search strings as an appendix is very useful.

f. To add clarity onto this review, I would suggest that PICO questions should be explicitly stated. For example, did the authors only look at certain populations? Adults, adolescents etc? what precisely were the exclusion criteria? Was it only related to different disease profiles?

g. When was the review conducted? What were the cut off dates? Were publications of all languages considered?

4. Data analysis and synthesis:

a. My understanding is that the authors refer to RR of adherence. Could odds ratios been more apprpriate? If not, why not? Give a reference.

b. I concur with the authors, that all lost to follow up would contribute to assumed non adherence using ITT approach, but this is the reason why the above statement (under outcomes) needs to be clarified.

c. would sensitivity analysis have been useful? Why or why not?

5. Results

a. PRISMA diagram should be included to show how studies were selected.

b. Interventions: I am unsure why social support measures feature here. do the authors suggest that calls, text messages etc contained social support, or were social support, or were delivered alongside social support? if the interventions were delivered outside or alongside mobile interventions in only one arm, then the results could be biased, and might not necessarily e measuring the effect of stand-alone mobile interventions.

c. The section titled study quality should probably just be renamed to publication bias. Quality involves other elements (eg recall bias, attrition bias, etc) which were not assessed and authors should add that as a limitation in the discussion.
6. Discussion

a. Authors rightly point out the limitation of RCT in not reflecting real world data. Could authors give a reference to the kind of pragmatic trials they refer to mitigate this issue? The authors have themselves excluded studies that were not randomised clinical trials, but it begs a reader to wonder if previous (and cited) reviews that gave shown more positive impacts included more observations (and therefore real world) circumstances.

b. line 590. I think that the authors need to be clear here that WHO may have depended on all kind of evidence, including non RCT studies to arrive at their conclusion. Did the authors look at the kind of evidence that WHO used? In addition, the authors might be are conflating text messages here with mobile as a delivery mechanism. If not, precisely what do they suggest WHO guidelines need to state?

I hope authors find these suggestions useful and look forward to seeing the paper published.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

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If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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