Reviewer's report

Title: Internet-based medical education: A realist review of what works, for whom and in what circumstances

Version: 3 Date: 27 November 2009

Reviewer: David A Cook

Reviewer's report:

The authors have done an excellent job of responding to comments. In particular, the added detail on the review methods will help readers unfamiliar with this approach. I have a few remaining concerns. I hope these comments are viewed as constructive, not critical. I appreciate and value what these authors have done. Still, I think there are changes that will make it better – to make the findings more robust and comprehensive, and perhaps to make the conclusions more acceptable to a wider audience.

1. First, I stand by my original concern regarding "the identification and selection of the "candidate theories." This process is described in very vague terms ("Browsing and snowballing" does not seem very rigorous. What sources were browsed? Were any experts contacted?). More importantly, it appears that several important theories were never considered – a major omission. The failure to consider important theories … limits the confidence I have in their conclusion that Laurillard and Rogers are the only theories with explanatory relevance."

I respect the methods of Realist Review, and although I have never attempted this I am familiar with the method and anticipate using it in the future. Again, I am aware that two authors are highly experienced in this field.

Nonetheless, I do not believe they were as rigorous as they could have been in identifying the candidate theories. To quote from an article by Pawson, Greenhalgh, and two other authors (Journal of Health Services Research & Policy Vol 10 Suppl 1, 2005: 21–34):

The reviewer must temporarily adopt a primary research rather than synthesis role and scavenge ideas from a number of sources to produce a long list of key intervention theories from which the final short list will be drawn up. An important initial strategy is discussion with commissioners, policy-makers and other stakeholders to tap into official conjecture and expert framing of the problem. [emphasis added]

Nowhere in the methods or results do I see that the authors made a serious attempt to scavenge ideas from a number of sources (in particular experts in the field) to produce a long list of candidate theories. I see a rather short list, and one
that omits many others that have been clearly identified as relevant, and that
have been discussed for at least 6 years in the medical education literature (and
much longer outside of medical education). The fact that they do not provide a
reference for Cognitive Load Theory (see page 17 of manuscript) and that they
cite an indirect source when referencing Mayer's Theory of Multimedia Learning
suggests that they are not familiar with these theories (although I admit this could
be over-inference).

The authors attempt to defend their approach by blaming the primary literature. I
do not believe this gets to the point. Simply put, the authors failed to consider
broadly several of the important theories in wide circulation over the past 15+
years. This doesn't invalidate their main conclusions (e.g. Box 1) at all, but it
does prevent them from stating that Box 1 is a complete summary of what is
currently known to inform "what works, for whom, and in what circumstances." I
realize much is unknown, and readily admit that most theories have been
inadequately tested, but the authors have failed to capture some of what is
known – that's the key.

This is not a fatal flaw, but a significant one. I would have preferred the authors
review their data again, considering at least some of the other candidate theories
I listed.

Again, this is not a fatal flaw, but must be acknowledged as a limitation.

Important: I do not believe that the statement that "Our review ... has shown that
the empirical literature has not, to date, explored these theories sufficiently to
provide testable data" is defensible. The authors cannot say this, because they
never tested these theories. At the very least, this sentence must be deleted.

2.

Second, I still would have liked a little more detail (quotes, examples) in the text
to help me understand how they arrived at their conclusions in the major sections
Technology Acceptance and Interaction. Again, I like the example/nonexample
appach used in Course-context Interaction. The Appendix is helpful, but most
readers will not access it. This would be nice; not essential.

3.

Third, I still have concerns with the wording in the paragraph contrasting virtual
microscopy for different learners. I actually like this paragraph a lot, but it must be
made clear that these are not the "same Internet-based application" but rather a
similar modality or technique (virtual microscopy) applied to a different course
and different learners. This is subtle, but important, because it raises the
possibility that it was more than just the learner level that affected the outcome
here; it could also be the instructional methods, barriers to access, hard to
find/use, etc (as you outline in Box 1). It gets back to the complexity of these
learning interactions, which we all agree are dizzyingly difficult to unravel! I
appreciate the desire to show a "direct link"; I think in this (and perhaps a few
other places) the link will need to be a bit more tentative.
4.

Finally, the Discussion devotes a lot of space (3 pages) to strengths and limitations. This seems excessive, and more importantly I do not think limitations were adequately acknowledged. In fact, most of this space is spent describing the strengths of the method and the weaknesses of the literature, rather than the limitations of the method.

I want to state clearly that I do not need to be convinced of the strengths or the rigor of the Realist Review approach – it is extremely useful, and the results can be just as valid as any quantitative (e.g. Cochrane) review. Of course, readers may need to be convinced, especially with a new method, so some elaboration on strengths is appropriate.

This being said, there are important limitations that attend any paradigm. As qualitative researchers, you will be highly aware that the researcher's biases are inescapable, and that the lens through which a setting is viewed will greatly affect what can be seen. I believe more attention should be paid to acknowledging the limitation of the Realist Review lens. As it stands I could find only one sentence (a partial sentence, in fact – bottom on page 16) describing the limitations of this method. A more balanced discussion of strengths and limitations would be welcome. Suggestion: Pull out the QUOROM statement (or the new PRISMA guidelines) and identify all the ways in which your review falls short using this yardstick. Openly acknowledge these limitations. Then explain how these are unavoidable, and how the strengths of the realist review complement and offset those of the quantitative review. I think readers will appreciate your candor, and better understand your position.

To specifically address the questions requested by the editors:

Major compulsory revisions:

The sentence in the Discussion, "Our review … has shown that the empirical literature has not, to date, explored these theories sufficiently to provide testable data." must be deleted or substantially revised.

All other suggestions could be considered Minor or Discretionary.

1. Is the question posed by the authors well defined?
   Yes, and it is an important question.

2. Are the methods appropriate and well described?
   Methods are appropriate, and well described, with the single exception noted above.

3. Are the data sound?
   Acceptable.
4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Some room for improvement as noted above, but nothing major.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

Acceptable, except for the one sentence in Discussion noted above.

6. Are limitations of the work clearly stated?

Most limitations are noted; see comment above.

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

As far as I am aware, yes.

The may wish to know that Robert Bernard (author of ref 4) published a new meta-analysis in Review of Educational Research this month, and they may wish to cite this.

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

Yes.

9. Is the writing acceptable?

Very well written overall. Very few, very minor grammatical errors (3, to be exact); a careful proofreading will solve this.

In closing, I wish to commend the authors for their efforts. I can appreciate what a monumental undertaking this must have been. It was a pleasure to review this manuscript. I hope you find these suggestions helpful.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

'I declare that I have no competing interests'