Author's response to reviews

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

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Author's response to reviews:

Dear Dr Norton,

Re: Manuscript number 2453225492755020

Thank you for giving us the opportunity to respond to the most helpful re-review comments from Professor Cook.

In light of his comments we have made further amendments to our manuscript and we hope that the manuscript is now worthy of publication. We would like to draw your attention to our response to Point 2 raised by Professor Cook and value your advice on the matter of the inclusion of additional boxes of data.

In the section below, I have provided our detailed responses to Professor Cook’s comments.

Yours Sincerely

Dr Geoff Wong

For and behalf of all authors

Reviewers'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.

OUR RESPONSE:

We agree that our coverage of candidate theories was not exhaustive, and we have now acknowledged this clearly in the manuscript. One problem, which we discuss at some length in the revised manuscript, is that the primary studies provided only limited data to test key theories. As no previous realist review had been undertaken in this field we were initially unsure about how much of this vast field we might be able to explain. Hence we focused on the most prominent demi-regularities that were present in this body of literature. What we found was that when we looked across all Internet based courses, two very prominent demi-regularities were whether learners were prepared to adopt the technology and the nature of interaction. In the revised manuscript we have made clear where further work needs to be done, and have flagged the other theories mentioned by Prof Cook.

It is perhaps worth emphasising that theory-hunting is an iterative process. In our original manuscript, we only mentioned 4 theories, but in fact we continued to test theories as we encountered them in the literature. You will now see in the Results section (under Candidate theories) that we have provided a list of all the theories we encountered. We looked for data to support each of these theories in our included studies and found this task to have been severely restricted by the data reported. We hope that the addition of this will now explain why we have devoted some words in our discussion section on reporting issues in our included studies.
In addition in order to explain the findings we found across the whole gamut of Internet based course, the candidate theories we used had to be middle-range in nature and this in itself would exclude some of the educational theories that might at first seem relevant because they are too ‘specific’.

We agree that had we consulted experts in the field widely, it is possible that we may have identified further middle-range theories and have now acknowledged this in the text. However, the recommendation to ‘consult widely’ in the original Pawson paper was directed in particular at reviews in which the authors are studying a field with which they are unfamiliar. Two of the authors of this review were relatively familiar with the field of Internet based education, though we acknowledge that we were not world experts!

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 approach used in Course-context Interaction. The Appendix is helpful, but most readers will not access it. This would be nice; not essential.

OUR RESPONSE:
We accept this point, and are very happy to include more detail. We ask the editors to advise us on whether a lengthening of the manuscript would be acceptable, especially as BMC Medical Education is a web based journal and hence Appendices are but a hyperlink click away?

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.

OUR RESPONSE:
Thank you for pointing this out. We agree that any associations that can be made do indeed have to be very tentative as what we are dealing with is trying to
‘predict’ human behaviour. We have revised this section to better reflect the tentative nature of any links.

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.

OUR RESPONSE:

We agree with your observation that realist reviews are best viewed as a form of theory driven qualitative systematic review method. As such it will ‘suffer’ from the limitations that any piece of qualitative research.

We have therefore revised sections of the discussion sections to point out in more detail the limitations that realist review shares along with other qualitative review methods.

We did not feel that this particular manuscript was the appropriate place to enter into a wider discussion about the pros and cons of realist review compared to other review method. We are actually working on another paper which addresses precisely this topic, and the problem we have is that to do justice to the topic we would need an additional 4000 words! We do however thank this reviewer in
particular for pushing us to an in-depth consideration of the strengths and limitations of realist review and hence inspiring what we hope will be a balanced theoretical paper on this topic, to be submitted shortly. One major offering in that paper will be a QUORUM/PRISMA style checklist for assessing the quality of realist reviews.

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.

OUR RESPONSE:
This sentence had been extensively 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.

OUR RESPONSE:
Thank you for directing us to this very relevant meta-analysis that seems quantitatively to support our qualitative finding on the importance of interaction.

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.

David A. Cook, MD, MHPE
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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'