Author’s response to reviews

Title: Health professions digital education on clinical practice guidelines: a systematic review by Digital Health Education collaboration

Authors:

Lorainne Tudor Car (lorainne.tudor.car@ntu.edu.sg)

Aijia Soong (aijia.soong@ntu.edu.sg)

Bhone Kyaw (bhone.mk@ntu.edu.sg)

Kee Chua (CHUA0728@e.ntu.edu.sg)

Naomi Low-Beer (naomi.low-beer@ntu.edu.sg)

Azeem Majeed (a.majeed@imperial.ac.uk)

Version: 2 Date: 27 Apr 2019

Author’s response to reviews:

Response to peer reviewer’s comments

Thank you very much for your comments which have helped us to further improve the quality and readability of our manuscript “Health professions digital education on clinical practice guidelines: a systematic review by Digital Health Education collaboration”. Please see below a point-by-point response to the comments and a description of the corresponding amendments to the manuscript.

Comment

Reviewer #3: The authors have responded to many of the comments I made in my previous review and the manuscript is now much clearer. However there are still claims made about the impacts of digital education for clinical guidelines made by the authors in the manuscript about which I have concerns. Inserting the terms ’probably or 'maybe' at some places in the text is not a sufficient moderation.

The authors state that for more than six studies they have concerns about study quality but the reader they cannot easily work out which studies these are in relation to the various synthesis that are conducted. Which analysis e.g. digital v no education, are the low quality studies in?

Response
We are pleased to hear that our responses and amendments were helpful. Many thanks for these additional suggestions. To reflect the quality of evidence in each comparison and for each outcome, we have now performed and included a GRADE assessment of the evidence with accompanying Summary of findings tables presented in the supplementary information. We included a corresponding comment on the quality of the evidence next to each outcome.

We have made the following amendments in the Methods (p 8 & 9, line 206 – 212):

“We assessed and reported the quality of the evidence for each outcome, using the following GRADE assessment criteria: risk of bias, inconsistency, imprecision, indirectness and publication bias. Two authors independently assessed the quality of the evidence. We rated the quality of the body of evidence for each outcome as “high”, “moderate”, “low”. We prepared 'Summary of findings' tables for each comparison to present the findings and the quality of the evidence (Supplementary information 2).”

Comment

Figure 3 - Meta analysis digital education v no intervention. There is a high level of heterogeneity indicated and acknowledged by the authors. This maybe driven by one of the studies as the authors state. However, they do not present analysis combining the other two studies to substantiate / support/ test the claim. To claim that digital education has a better outcome than no education (probably or not) is a questionable claim in these circumstances (i.e. questions about study quality and high levels of heterogeneity and very few studies.) Authors may wish to make that claim but I think they need to more to explain why they think this is reasonable. (see point below about long term and change score results)

Response

Thank you for your comment. We now explain in the text that the poor overlap between this one study CIs and the remaining two studies’ CIs demonstrates that the heterogeneity is driven by this one study. Please see below for a forest plot of the two remaining studies to substantiate our claim.

We have now removed “probably” from the paper and added the following sentence to this paragraph to explain this further (Results, line 270 – 273, p 11):

“The high observed heterogeneity was largely driven by a study on spaced education via emails showing large improvement in the intervention group (SMD = 1.52, 95%CI: 1.06, 1.97) and CIs that poorly overlap with the CIs from the other two studies in this analysis.”

Comment

I think the reporting of the immediate post test scores, longer term outcome scores and the change scores in separate sections is confusing. It fragments the evidence about the impact of the
same intervention into different places. For example the outcomes for Digital education compared to 'no education' should be all reported together i.e. immediate post test scores, long term scores and outcome scores - This would provide a much clearer indication of the strength and consistency of the evidence from each individual studies and overall.

Response

Thank you for your comment. We have now added this information within the relevant section.

In “Digital education vs no intervention” section we added the following text (Results, p 11, line 274 – 277 as well as p12, line 290 - 294):

“One study measuring long-term knowledge retention at six months post-intervention [45] reported moderate beneficial effect of digital education intervention group when compared to no intervention (SMD = 0.73, 95% CI: 0.09, 1.38).”

“The same two studies also reported long-term data for behavioural change outcome. The follow-up behavioural change findings in these studies were consistent with those immediately post-intervention with one study evaluating an online module reporting no difference between the groups at six months [45], and the other study on spaced education still favouring the intervention group at 18 months post-intervention [38].”

In “Digital education vs traditional learning” section we added the following text (Results, p 12, line 304 – 207):

“Three studies also measured long-term knowledge retention one to six months post-intervention and reported no difference between the groups in two studies [35, 41] and moderate improvement in digital education group in one study”

In Digital education (more interactive) vs digital education (less interactive) we added the following text (Results, p 14, line 335 – 338):

“One study reported moderate improvement in knowledge growth retention at 30 days follow up in the more interactive form of digital education intervention compared to less interactive one (SMD = 0.63, 95% CI: 0.01; 1.24) [40]. The same study reported higher satisfaction in the more interactive group at follow up.”

For the pre-post intervention change data, we added the following sentence to the Limitation section as follows (Discussion, p 17, line 425 – 429):

“In studies reporting pre-post intervention change data, we extracted post-intervention data to ensure consistency in the presentation of findings across the studies included in this review. Studies with pre-post intervention change data reported an improvement from the baseline but the findings were in all cases consistent with the post-intervention data.”
Comment

For the meta analysis Digital education v traditional education (also figure 3) the authors claim that this shows that digital education is 'as effective' as traditional education. However it is not clear how they reach this interpretation. The point estimate in the pooled estimate of effect favours digital education. The 95% confidence interval for this pooled estimate crosses the line of no effect. So it is unclear how this can be interpreted as both types of education as being equally effective.

Response:

Thank you for your comment. We have now rephrased this into “small statistically non-significant beneficial effect on knowledge scores in the digital education group” (please see here for an example of a systematic review with similar interpretation) and “digital education is at least as effective as traditional learning”. This is based on the following:

• the pooled estimate shows a small effect size (0.23)

• the 95% CIs of the pooled estimate cross the line of no effect (statistically non-significant)

• the lower 95%CI of the pooled estimate (-0.12) does not reach the small effect size cut off line (0.2 as defined in our review).

Although the improvement is non-significant, the lower limit of the 95% confidence interval (-0.12) is very small and hence even if (in the worst case scenario) digital education is not quite as good as conventional education, the difference between the two is not of any practical significance.

We have downgraded the quality of evidence for this outcome to moderate due to imprecision.

Comment

With regard to the comparison between more or less interactive digital learning the authors conclude that higher interactivity and engagement may lead to larger gains but it is not clear what data they use to arrive at this claim. I think it is partly at least data from figure 4 but a) this is not made clear b) I cannot see any data in figure 4 that would support this interpretation.

Response:

This claim is based on a trend that was observed in all the included data as presented in the Discussion section, 2nd paragraph and is also reflected by high attrition reported in studies on less interactive intervention (3rd paragraph Discussion). It is also based on the fact that the only observed improvement in behaviour outcomes and patient outcomes was found in a study with an interactive form of education using email delivered spaced (game-based) education. This
statement is not informed by figure 4 which serves as a visual summary of all behaviour outcome
data found in this review.

Comment

There are many outcomes drawn from the same studies. Firstly the authors should make clear
that all of the different comparisons made in an individual study are independent i.e. not from the
same subjects. To analyse the data in figure 4 I assume some kind of vote counting has been used
but the reader needs to know the details of this. Narrative synthesis is referred to in the text but
what exactly this means in terms of process is not described.

Response:

Thank you for your comment. The purpose of the forest plot is not to analyse the data but to
provide a visual summary of the outcome data as presented in the Table 1 and narratively
synthesized in the Results. This forest plot has now been moved to the supplementary
information under caption “Forest plot of all behavioural change outcomes reported in included
studies without the pooled estimate”. As this information is already presented in the text and the
table, we are happy to remove this forest plot altogether should you feel it is not helpful. We
have now clarified the purpose of this forest plot further as follows (Methods, p 9, line 212 –
220):

“We were unable to pool the data statistically using meta-analysis for some outcomes (e.g. skills,
behaviour) due to high heterogeneity in types of participants, interventions, comparisons,
outcomes, outcome measures and outcomes measurement instruments. We presented those
findings in form of a narrative synthesis. We organised the studies by the comparisons and
outcomes. We transformed the data expressed in different ways into a common statistical format.
We tabulated the results to identify patterns in data across the included studies focusing on both
the direction as well as the effect size where possible. In addition, we displayed all the available
behaviour change outcome data in a forest plot without a meta-analysis as a visual summary (see
Supplementary information 3). In some studies behaviour was measured using different
approaches and tools. Instead of selecting one outcome or statistically pooling the data to
produce a single estimate per study, we decided to present all behaviour change outcome data
from the included studies as it focuses on different aspects of clinicians’ behaviour and practice.”

Comment

On page 8 states ' subgroup analysis not possible due to the limited no of studies in subgroups,
comparisons and outcomes - yet they have in effect done subgroup analysis by dividing studies
into 3 categories - I assume they mean subgroups based on different outcomes/ comparison /
study quality etc - this should be made clear

Response:
Thank you, we have now amended this as follows (Methods, p 8, line 200 – 201):

“Subgroup analyses were not feasible due to the limited number of studies within respective comparisons and outcomes.”

Comment:

On page 9 They state that they present data from studies ’unable to pool’ (I presume they mean they did not meta analyse for some reason which they should explain ) but is this figure 4? or somewhere else?

Response:

Thank you for your comment. We now explain this more thoroughly in the Methods section as follows (Methods, p 9, line 212 – 220):

“We were unable to pool the data statistically using meta-analysis for some outcomes (e.g. skills, behavior) due to high heterogeneity in types of participants, interventions, comparisons, outcomes, outcome measures and outcomes measurement instruments. We presented those findings in form of a narrative synthesis, organised by the main comparisons of the review. We present the direction as well as the effect size where possible. In addition, we display all the available behaviour change outcome data, originating in some studies from multiple measurements, in a forest plot without the pooled estimate as a visual summary (see Supplementary information 3).”

Comment:

Throughout the text there is interchangeable use of the terms ‘digital learning’ and ‘digital education’. I could not ascertain the reason for this or a consistent pattern.

Response:

Thank you for highlighting this inconsistency, we have now amended this to “digital education” throughout the manuscript.