Reviewer’s report

Title: A counterbalanced cross-over study of the effects of visual, auditory and no feedback on performance measures in a simulated cardiopulmonary resuscitation

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Reviewer: Dave Hostler

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This study works to sort out the effects of audio and visual real-time feedback during one-rescuer CPR performed on a manikin. Direct measurement of compression force, lactate, and perceived exertion are notable additions to this area of the literature. The investigators are to be commended on taking these added steps.

One study design issue is that the authors did not test combined audio and visual feedback which is the modality currently offered in clinical devices. While the audio portion can be disabled in the clinical setting but it is not commonly done (see Resuscitation Outcomes Consortium feedback study recently published in BMJ). It makes it harder to frame the study. Is this being approached from an education (eg the merits of certain types of feedback) or clinical (performance and fatigue) standpoint?

Major compulsory revision/consideration: The second major issue is why the investigators chose 10 minutes of CPR. AHA guidelines and the literature almost uniformly recommend frequent change in rescuers performing chest compressions. Arguably, there are 7-9 minutes of irrelevant data influencing your conclusions.

My specific comments are:

Minor essential revision: The abstract is misleading in a few places. It is inaccurate to put 15 subjects in the abstract when only 10 provided complete data. It is also unclear that there were two visits in Phase 2 until the reader gets to the results. The last sentence beginning with “The perfect confounding…” is a concept that has no setup earlier in the abstract. Its meaning is not appreciated until late in the discussion.

Background:

Minor essential revision: The sentence in the first paragraph ending “…feedback does not provide the same improvements in chest compression depth” is not true. Both Kramer-Johansen and the recent ROC study demonstrated deeper compressions with feedback.

Major compulsory revision: The last two sentences of the second paragraph are
unreferenced and do not support the conclusion of that paragraph ending "…spring loaded chests of resuscitation manikins". It has been argued that manikins do not provide the same resistance as cardiac arrest victims.

Methods:

Minor essential revision: It is not clear if all 15 subjects were included in the no feedback group or if only the 10 subjects completing all phases were analyzed.

I am particularly pleased to see information included about the subjects morphometrics although the broad age range in such a small cohort could be introducing variability.

Minor essential revision: Presumably, every subject had current CPR cards, not more than 2 years old, but did you determine how long it had been since the last refresher? Was any formal or casual refresher instruction provided?

Minor essential revision: Were specific, written instructions provided on anchors and use of the Borg scale? Since the design always placed the no feedback condition in front of the feedback conditions, this could create bias in how the subjects used the scale making it difficult to interpret these data.

Major compulsory revision: There is no indication a priori or post hoc power calculations or how the loss of 5 subjects changed the power to detect differences.

Discretionary Change: Biomechanical data beyond force were not presented making figure 2 unnecessary. Figure 1 also has limited value.

Results:

Major compulsory revision: In general, the data make me believe there is an outlier in the audio feedback group that is influencing the analysis and its interpretation.

The presentation of Table 1 in the text is difficult to follow and differences were not annotated on the table itself. My comments are based on my understanding of the table after I annotated it from the text.

Discretionary Change: In general, I would prefer to see SD and not SEM.

Major compulsory revision/consideration: If percent correct compressions were not different between groups they why argue visual feedback is better? Likewise, the key component of depth was not different between audio and visual feedback.

Discretionary Change: Rates below 100/min with feedback differs from most other manikin studies of feedback. Are there insights for this observation?

Major compulsory revision: Is 22 +/- 6 for incomplete release in the audio feedback group correct? If so, I would guess there is one subject acting as an outlier and is skewing the data.
Discretionary Change: Figures 3, 4, 5: The scale on the x-axis is not intuitive for a time sequence. I would recommend whole or half minutes. Six superimposed lines are difficult to interpret. I would recommend only using the linear trend lines.

Major compulsory revision/consideration: The figures of force and rate suggest that audio feedback is worse than no feedback 15 seconds into the bout. If there is not a subject dragging the trend lines down in the audio group then you are almost obligated to argue that audio feedback alone is harmful.

Discretionary Change: Given that the study is of chest compressions in a manikin, it is not surprising that depth and force co-vary. This probably doesn’t have much impact on actual resuscitation where chest wall compliance is dynamic over time but should be noted in the paper.

Discretionary Change: The two-minute analyses are probably more relevant for clinical application. In my opinion, this would be a stronger paper if you concentrated on this epoch of the data.

Major compulsory revision/consideration: There is a ‘b’ superscript in Figure 8 not detailed in the figure legend. Based on the text, I assume this means the no feedback RPE was higher than the feedback conditions. This is counterintuitive. If feedback is compelling greater performance, why would there not be greater perceived exertion?

Major compulsory revision/consideration: The limitations section is absent.

Overall, I think there are interesting data here and with revision, this will make a contribution to the literature but I am not sure I agree with the interpretation of the data as they are presented. In the grand scheme, I would interpret these data as meaning feedback (regardless of type) improves CPR performance (overall correct compressions and compression depth). The fatigue data are more difficult to interpret and may be hurt by insufficient cohort size.

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

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'