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Letter to the Editor

Letter to the editor on the article "Virtual reality simulator versus box-trainer to teach minimally invasive procedures: A meta-analysis"

Dear Professor Lau,

We took considerable interest in reading the article by Guedes et al. [1] entitled "*Virtual reality simulator versus box-trainer to teach minimally invasive procedures: A meta-analysis"* published online in December 2018.

The authors conducted a meta-analysis to evaluate the effectiveness of virtual reality simulator (VRS) training compared to box-trainer training (BT) in minimally invasive surgery (MIS) techniques.

While its attention-grabbing title and bold conclusion command great attention, there are several ambiguities we were hoping to clarify. We endeavoured to appraise this meta-analysis in an objective and sensible manner.

Firstly, the authors appear to have referenced the same study – as reference 8 and 16. Compounding to this, the study referenced as 8 was included and evaluated in the meta-analysis whilst conversely, the same aforementioned study, now referenced as 16 was excluded *"because the data needed (mean and standard deviation) was not available"*.

Further remarks can be made with respect to some fundamental aspects of the article.

The authors did not define what they refer to as *"minimally invasive surgery (MIS) techniques"*. There is no mention or reference to what this entails, unfortunately leading to uncertainty in establishing the scope of the study and thus its relevance and clinical basis.

Similarly, the lack of clarity in defining the article's primary outcome of *"performance score"* had comparable effects. Thirteen out of the total twenty studies described in Table 1 "*Descriptive analysis from the selected studies*" [1] were not classified in terms of performance score. Only seven studies had a performance score parameter (GOALS, OSA-LS, OSATS) and of those seven, two were indistinctively labelled as *"Other"*. Although this limitation is explored in the discussion, we query the methods the authors used to assess participants' performance when no scoring system was named or identified.

Similarly, while all VRS models used were named and specified, only ten out of the total twenty BT models were identified (also in Table 1 [1]). Some of these studies, for example, Tanoue et al., were erroneously labelled as *"not specified"*.

Also worthy of mention is the method by which the authors categorized *"participant experience"* into two groups – as either *"novices"* or *"participants with no experience"*. Unfortunately, the authors omitted to clarify this distinction; this is especially relevant since the eligibility criteria reports that "*participants included medical students, doctors, resident doctors of surgical specialties and surgeons, independent of their experience with MIST*".

Likewise, the authors did not clearly address what was designated as either *"basic tasks"* or *"advanced tasks"* – making it difficult to ascertain whether *"advanced tasks"* connote *"running the tissue"* only or imply other tasks too.

We caution the interpretation of *"time to complete peg transfer"* as the parameter used in demonstrating the superiority of VRS training vs. BT training. As the authors rightly mentioned in the discussion, using *"time to complete"* to measure performance is limited and flawed.

We commend the authors Guedes et al. for their thought-provoking meta-analysis.

We believe that in order to further explore this subject matter, studies examining well-defined participant groups, clear outcome measures and larger sample sizes ought to be undertaken.

Data statement

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Ethical approval

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C Bojanic contributed to writing the manuscript in consultation with K To.

Conflicts of interest

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Reference

[1] H.G. Guedes, Z.M. Câmara Costa Ferreira, L. Ribeiro de Sousa Leão, E.F. Souza Montero, J.P. Otoch, E. Luiz de Almeida, Artifon Virtual reality simulator versus box-trainer to teach minimally invasive procedures: a meta-analysis, Int. J. Surg. 61 (2018) 60–68, https://doi.org/10.1016/j.ijsu.2018.12.001.

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