Reviewer’s report

Title: Improving clinical judgment by simulation: A randomized trial and validation of the Lasater Clinical Judgment Rubric in Chinese

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Reviewer: Carl Thompson

Reviewer's report:

Abstract - not sure that "nourishing" is the appropriate phrase here
If its nursing judgement why sample students with, "Chinese medical skills in practicum". Suggest delete or clarify link to nursing

Introduction:
Line 6 : "Clinical judgment is the ability to make decisions based on different types of knowledge". There are more formal definitions of clinical judgement out there. The working definition offered (unreferenced) suggests judgement as a process that includes decisions. I favour judgement as the assessment between alternatives (whereas a decision is the choice between alternatives). Others (Higgs) offer judgement as a process akin to the use you have offered. I prefer for the separation of the two distinct (but linked) processes (judgment and decision making).

Line 24 The argument that, "Due to limited training in the hospital environment, it is difficult for nursing teachers to use traditional measurement tools to evaluate students' clinical judgments[5]." Does not immediately make sense? What are these measurement tools? Why do hospital training limitations preclude them?

The aim of validating a well established tool in a Chinese context is sound and appropriate.

The final paragraph's English does not read well -"simulation has not been experimented [suggest shown] to improve clinical judgement in China, this study was also aimed [aims] to compare simulation teaching with traditional teaching with CHINESE nursing students in the Chinese culture, with the OUR hypothesis is that that the former outperforms the latTer".

The hypothesis could be even more specific "Our hypothesis is that simulation leads to improved clinical judgement when compared to traditional (non simulation based) teaching methods."

Methods:
Whilst randomisation is welcome to reduce the effects of confounders and offer a more robust evaluation (comparatively rare in nurse educational research); the authors have omitted to outline a basis for the eventual sample size (what size effect did you expect? What level of power did you want
to have confidence in the eventual effects? How many students were needed to trust the results?). What was the randomisation method? Any checks on whether the randomisation was successful? Suggest you use standard reporting guidance for RCTs http://www.consort-statement.org/ to ensure that all the methods and choices are clear.

There is not enough detail on the intervention and control groups and associated exposures. Suggest that the TIDIER guidelines http://www.equator-network.org/reporting-guidelines/tidier/ are used to report the intervention. As it stands it would be almost impossible to reproduce the experiment with any fidelity. Doing so, will provide far more useful findings for the reader. As an example, imagine you were not one of the study authors and you were to read the following from your paper:

"All students were divided into groups with five or six in each for the simulations. Each simulation took one hour including operation, self-evaluation, teachers' evaluation and reflection. The simulation course proceeded with the order of pre-learning activity, simulation operation, and writing reflection diaries. Each student participated in the three simulation sessions with different designs. Students in the control groups were given demonstrations of operations, followed by the students' practices, as is referred to the conventional teaching in this study."

Could you replicate the content as well as the format?

How did you ensure fidelity in the intervention delivery for the 2 x 2 groups (e.g. intervention group time 1 and intervention group 2 time 1??) . How do we know that the intervention was consistently delivered?

Was there a baseline measure? If not, then it is vital that we know that randomisation actually worked (i.e. groups didn't differ on known [but crucially, unknown] factors?

I am not a statistician but the analysis appears well specified. However, in RCTs the statistical elements are often less important than the execution of the design itself. I would prefer to see more attention paid to reporting overall than the minutiae of Bayesian modelling. (which can be handled with online appendices or references to more detailed expositions). Why not, undertake a simple (i.e. conventional frequentist analysis of variance) approach to testing for differences between groups though? Was it because an apriori sample size calculation was not carried out?)

Table 1: should be confidence intervals not credibility intervals I think

Discussion: It is not surprising that none of the "age, gender, and classes" (sic.) impacted on the judgement; why would they? Were you expecting men to think differently to women? Slighty older people to judge differently to people a few years younger than themselves? If you wanted to build in a priori variables likely to impact on judgement then "prior experience with healthcare" would have been more appropriate perhaps.

You should provide the ethics committee approval number

Summary:
I like the fact that the team attempted an RCT, with the commensurate ability to draw more robust conclusions. However, as an RCT, it is reported so poorly that I cannot reliably link conclusions to analysis and execution. However, the work seems to have merit and I am sure that if the conventions of trial reporting were followed then, a) the team would find it easier to present the required detail to
foster trust in the results and b) the reader would find it considerably easier to replicate and develop any significant (educationally or statistically) findings and promising angles revealed.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

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