Reviewer's report

Title: Integrated Problem-Based Learning in the Neuroscience Curriculum- The SUNY Downstate Experience

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Reviewer: Enoch Kwizera

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General
INTEGRATED PROBLEM-BASED LEARNING IN THE NEUROSCIENCES CURRICULUM –THE SUNY DOWNSTATE EXPERIENCE

Author: Brian Trappler

1. Is the question posed by the authors new and well defined?

The question might be ‘new’ in as far as it relates to a ‘neurosciences’ curriculum, but ‘integrated problem-based learning’ as such is not new, and has been practised in medical education for nearly 30 years. The question is vaguely posed / defined. One would expect the teaching / learning of Psychopathology (problem-based or not) to be, by nature, integrated with other neurosciences, particularly neuroanatomy, neurophysiology, psychopharmacology, and to a lesser degree, clinical neurology; and it is not clear from the manuscript whether this was or was not the case before the reported changes took place.

The statement of the problem is also compromised by the author’s implicit biased negative view of PBL, wherein, in the first paragraph of the introduction, he repeatedly cites a 13-year old reference reviewing literature up to that time, and somehow fails to balance this with what has been published since 1993 in this field -e.g. Iputo & Kwizera (2005) who have reported that PBL improves the academic performance of medical students in South Africa. There also seems to be discrepancies between the article title, the research tool (questionnaire), and what the author stresses in the discussion and conclusions. The 11-item questionnaire has 5 items relating directly to the content and/or process of lectures, as opposed to 4 on PBL. And then in reporting the results and in the discussion, emphasis shifts to the supposedly positive role of content experts in PBL . In view of all of the above, the title is misleading.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

Methods are described satisfactorily, but because this is a report of a unique experience in unique circumstances, the question of replication probably does not arise. Possible shortcomings of the questionnaire have already been alluded to, as have the discrepancies between the research problem, the research tool, and the manner in which the results were discussed.

3. Are the data sound and well controlled?

Not quite. The research tool (questionnaire) was flawed, not only in content bias (more items on lectures than on PLB) but also in detail. Without giving respondents a better opportunity to express their opinions (e.g. with a Likert scale), the questions are ‘leading’.
The third and forth columns of the results table refer to “analysis of differences across age groups”, but I do not recall encountering these categories in the methods or anywhere else in the paper. It is therefore difficult to infer any conclusions from this aspect of the data. The author also has the tendency to report subjectively (e.g. “many students”) which does not help the reader grasp the importance of the result in question.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Difficult to respond to this, in view of the foregoing. In particular, I am concerned about the author’s tendency to report results by inference, and not by direct measurement. For example, on page 8: “This is reflected in Question 4 of the Table”; and on page 9: “This was reflected in the students’ responses to Questions 5 and 10 in the Table”.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

No. They are biased against PBL, particularly PBL sessions not facilitated by a content expert. And for this the author cites old references and totally ignores more recent ones with contrary results, thus losing balance. Content experts and student satisfaction aside, it important to note that the students’ test scores were not significantly different in the two cohorts used in the study; but this does not receive the prominence it deserves in the discussion.

The author also fails to mention limitations of the study, among which I can count: 1. This seems to have been a top-down curriculum change in which (if I read it right) the Dean ‘decided’ and it is not clear what consensus (if any) was there among Faculty about the curriculum change. There is no mention of internal dissenters (possibly including the author) who had to go along against their prevailing beliefs and / or misgivings. 2. There was apparently no tutor training or similar preparation of tutors, nor any preparation of the students for the change. 3. The “integrated PBL” reported here was limited to one (relatively small) part of the medical curriculum i.e. psychopathology, and all else being well, it might still not be prudent to extrapolate findings to the entire undergraduate medical training curriculum. 4. Lastly, the implementation of the reported curriculum change was rather sub-optimal, and as a result, failure or some degree of failure was inevitable!

6. Do the title and abstract accurately convey what has been found?

No. As already mentioned, the title is misleading, particularly when read in conjunction with the methodology, discussion, and conclusions. At the end of the day, the author is telling the reader that integrated PBL is no good, and if it is to benefit the student at all, it should be facilitated by content experts.

7. Is the writing acceptable?

Yes, to a great extent. I however have a problem with a single author repeatedly saying “we” (unless it is the Royal ‘we’ …); and there are a few syntax errors (e.g. on page 6, ‘principals’ instead of ‘principles’).

8. Recommendation

In view of all the foregoing I make no suggestions for any revisions, and I recommend that this manuscript should not be published.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Discretionary Revisions (which the author can choose to ignore)

What next?: Reject because too small an advance to publish

Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal

Quality of written English: Needs some language corrections before being published

Statistical review: No

Declaration of competing interests:

I DECLARE THAT I HAVE NO COMPETING INTERESTS (notwithstanding the citation of my own publications in this review)