Reviewer's report

Title: Hospital Safety Culture in Taiwan: A Nationwide Survey Using Chinese Version Safety Attitude Questionnaire

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Reviewer: Andrew Symon

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This is an interesting paper, reporting the administration of the Safety Attitude Questionnaire in Chinese (SAC-Q).

The method of data analysis is not my strength, and would need to be reviewed by someone more familiar with this.

I have a number of queries which I believe the authors will need to clarify before this can be accepted for publication. I would classify all the following as Minor Essential:

1. The authors organized the administration of this survey tool among Taiwan healthcare workers. While it is stated that 45,242 questionnaires were received (an impressive figure), there is missing information regarding the process. 208 hospitals were initially involved, but eight withdrew; it is not clear how many hospitals declined to take part altogether. As the authors note in the Limitations section, this may have introduced a source of bias, but the reader has no way of knowing how big a source of bias this might have been without knowing something of the non-respondent hospitals. Were they bigger or smaller than respondent hospitals? What were the reasons for not taking part?

2. There is also the issue of response bias between and within the hospitals. Did some hospitals or some occupational groups have much higher response rates than others? The authors should clarify how many hospitals were approached in total. We know that 528 administrators were approached, but not how many hospitals they represented. It would also be helpful to know what the response rates were by occupational group (i.e. what percentage of doctors responded? etc)

3. In the Background section the authors state that they aimed “to develop a valid safety culture survey instrument in Chinese and to measure hospital safety culture”. This seems to conflate two processes: developing and testing a tool, and carrying out a large scale survey. It seems to me that they translated an existing tool, rather than developed a new tool; and while they tested the psychometric properties, the possibility of response bias could call into question how robust that was. I appreciate that, given the overall response rate, this was in fact unlikely to be a problem, but I believe it should be acknowledged as a possible limitation.

4. In the Methods section, the removal of the stress recognition dimension needs
to be discussed more fully, as this could have affected the overall structure of the tool, and makes international comparisons difficult.

5. Also in the Methods section, the mechanism for measuring the staff work experience needs to be explained: was it the same kind of Likert scale used elsewhere?

6. The abstract states that the survey was carried out in 2007 and 2008. However, on page 8 it is stated that the survey was carried out in May-June 2008.

7. I could not tell from these results whether the relevant occupational group was excluded from the analysis of the extra measures. 71.1% of respondents perceived good collaboration with nurses, but 65% of respondents were nurses. What results do they get if that occupational group is excluded? Both ‘with’ and ‘without’ results are interesting.

8. It would also have been interesting to get the range of scores among the hospitals: was there much of a range? And if so, what does this tell us?

9. In the Discussion section I expected to see some consideration of the possibility of bias, either from a skewed sample of hospitals, or a skewed sample of occupational groups.

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

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

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.