Reviewer's report

Title: Belief system, meaningfulness, and psychopathology associated with suicidal behaviors among Chinese college students: a cross-sectional survey

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Reviewer: Robert Young

Reviewer's report:

The manuscript has improved considerably in several areas, and the authors have addressed many of the concerns raised by the reviewers. However, I think it still requires some attention in a few areas. That said, the study appears to be fundamentally sound, well conducted and reports some important and interesting findings from a non-western perspective. Accordingly, I would encourage the authors to continue with the submission, which should result in a manuscript with some very interesting and potentially important findings.

Minor/Major Essential Revisions

1: Language: I realise this may be a difficult aspect for non-native English speaking authors, but there are still several awkward sounding sentences, flow, word choices and superfluous sentences in the manuscript. Curiously, several sections/paragraphs flow well, e.g. the description of Confucianism. I wonder if two key authors are involved in the main writing!

2: The manuscript is still verbose in parts and could be condensed considerably. If the discussion focused on only the key points this could be easily achieved. All of the points discussed are relevant, they just seem to be over-elaborated. There are parts that may require further explanation for a western audience. For example before further investigation, I did not know what ‘Deng Xiaoping Theory’, ‘Important Thought of Three Represents’, and ‘Scientific Outlook on Development’ meant?

3: Suicidal behaviour vs. suicidal ideation: I agree with my fellow reviewer that it is a mistake to label suicidal thought and cognitions as suicidal behaviour. One solution would be to use different term: suicidality or suicide risk as a term to encompass both thoughts and behaviours.

Minor Essential Revisions

Analysis – the authors have made considerable improvements in the analysis, but there are a few issues that need resolved. Given most of the models should be set up this should take only minimal effort to address.

4) Missing data: The authors rightly test the models using only complete data. Unfortunately, deleting incomplete data is one of the poorest methods to address missing data issues (see http://missingdata.lshtm.ac.uk) and by doing so the authors, miss the opportunity to use one of AMO’s best and easily implemented features. Using the full-information maximum likelihood option, is simple to use in
Amos, would increase statistical power and at least partially address missing data issues.

5) The authors select the unconstrained multi-group model as their ‘final’ model despite some obvious similarities and differences in gender – now an important feature of the paper. The authors demonstrate an ‘overall gender difference’ in the multi-group model. The key question we need to address is this - which of the pathways are the same across genders and which show a gender difference? The unconstrained model and the modification index available in Amos could be used to identify the pathways that differ most. For example, the pathway from Religious belief to Psychopathology is likely to be significantly different for males and females; in the constrained model, its modification index should be significant, as should the pathway from Political belief to psychopathology. Freeing these and other key constraints could result in a better fitting final model.

6) It is good SEM practice to include factor loadings in SEM figures. For the purposes of replication and transparency - if you use a variable in the model you should report its loadings. The authors could sidestep this by supplying a supplementary figure with all loadings (including SCL-90 subscores), while retaining current simplified figure in the main paper.

7) A similar position is true regarding the reporting of correlations (including SCL-90 subscores means & SD) and/or the variance-covariance matrix. The authors have now used separate male and female datasets and so should now supply a table for each. Again, these do not have to be included in the main manuscript and could be given in a supplementary table. Fundamentally, the table must contain ALL the information (including Means and SD for binary data) necessary to replicate the models. The current table does not contain the information required.

8) The manuscript requires some further condensing and ‘tiding up’. For example decimal points could be reduced where appropriate (e.g. results to 3 decimal places are not required when reporting Chi-square or AIC).

**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, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.