Reviewer's report

Title: A school-based resilience intervention to decrease tobacco, alcohol and marijuana use in high school students: a pilot study

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Reviewer: Martin Wong

Reviewer's report:

The authors studied whether an interventional programme can enhance the resilience level and protective factors in school settings, and evaluate its impact on the level of substance abuse among adolescents. This is a timely manuscript and is relevant for school health research. It also bears a significant policy implication.

I have the following comments:

Major Compulsory Revisions:

1) In the abstract the design of the study is not too clear. Is this a RCT or a quasi-experimental intervention with no control group? It should be stated in the methods section.

2) The authors have already mentioned a lot of studies related to school-based programmes, and that many of the trials are controlled (references 9, 23, 32, 38, 46). Although the results are not consistent, it is unknown why another uncontrolled study will be justified. I recommend the authors suggest what is unique about their study (& which has not been conducted before) to put the study justification into context.

3) In the methodology, the settings will need to be further elaborated. For instance, what is meant by “one local government area in New South Wales”, and what are the characteristics of this region? How did this region compare with other regions in the NSW in terms of demography other than socioeconomic status? The readers will need this information to assess the generalizability of the findings.

4) Have there been any student movement to other schools between the calendar years? Also the ethnicity and cultural background of the students will need to be reported as this could be an influential factor determining their response to programmes and survey applicability.

5) The improvements in resilience scores (0.17) and protective scores (0.42) seemed modest, despite the statistical significance. Hence the authors should describe what they perceive these modest improvements mean in real-life school programme stakeholders.

6) The first paragraph has described the implications of conducting a controlled study. However, critics might argue that even before this study, that implication is already apparent given the numerous studies with inconsistent results. I
recommend the authors to think about what is the most crucial implication from this study which will have impact on the current school health academia.

7) The reduction in the youth risk behavior requires some caveats in interpretation: were the students aware of the purposes of such intervention which could bias their self-reported responses when the surveys are completed?

8) One could reasonably speculate that the responders are more likely drug non-users. This issue will need to be discussed.

Minor essential revisions

1). For Table 2, simple statistics (chi-square) should be provided for the readers to report whether the 2002 and 2006 characteristics were different. In addition, comparing only the grade and gender is not sufficient to claim homogeneity of the two groups (2002 vs. 2006 cohort). Are there other parameters where the authors would like to report and compare?

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

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**
'I declare that I have no competing interests'