Author's response to reviews

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

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Author's response to reviews: see over
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Dear Dr Silvia Martins

Thank you for providing the reviewers comments on our manuscript titled “A school-based resilience intervention to decrease tobacco, alcohol and marijuana use in high school students: a pilot study”.

The reviewer comments have helped clarify the strengths of the study, its interpretation and opportunities for further research. We appreciate the opportunity to address these and resubmit our manuscript for your consideration. Please find attached our responses to the reviewer comments.

We hope it meets with your approval and look forward to receiving your response

Yours sincerely

Rebecca Hodder
Program Manager
Hunter New England Population Health
Reviewer: Martin Wong
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.

We agree and have amended study design statement in the abstract (page 2).

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.

At the time of development, no published studies could be located that aimed to address the focus of the study: reduced adolescent health risk behaviours; implementation of a comprehensive resilience and protective factor intervention; explicit and comprehensive program adoption strategies.

Further, those that had addressed one or more of these issues, variable outcomes were reported, as described. Given this absence of prior evidence addressing these research design dimensions, the pilot study was implemented to test whether an intervention addressing these elements was potentially efficacious. Pilot studies are recommended in such contexts prior to the conduct of more rigorous controlled study.

The final paragraphs in the introduction section have been modified to more clearly reflect this evidence-based logic for the study (page 6).

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.

The settings and sample sections of the manuscript has been amended to clarify the geographic region in which the schools were located, and to include additional details regarding the regions demographic profile (page 7).

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.

Whilst data suggests that a proportion of students change schools each year across the state, the extent to which the rates of such movement occurred in the study schools is
not known. During the study period the number of students increased in two of the three schools. Whilst students leaving the school during the study period would not be expected to have an impact on the study outcomes, the entry of new students to the schools during the study period has the potential to have had an impact due to reduced exposure to the intervention. As the effect of this would be to diminish the effect size of the intervention, the reported results could be considered to be a conservative estimate of effect. Information to this effect has been included in the discussion (page 17).

No data were collected regarding student ethnicity or cultural background. This characteristic of the study and the need for inclusion of such information in future studies has been added to the discussion (page 18).

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.

Further discussion regarding the modest improvements in resilience and protective scores have been included in the discussion (page 15).

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.

The discussion has been amended to reflect this more clearly (page 14).

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?

Students were blinded to the study aim of decreasing health risk behaviours and this detail has been added to the methods section of the manuscript (page 7).

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

This issue is addressed within the discussion and has been amended to include more detail (page 18).

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?

The chi square statistics have been added to table 2.
Reviewer: Asha Goldweber
Major Compulsory Revisions
1. Overlooked seminal research by Mark Greenberg, Richard Catalano, and Suniya Luthar, respectively. Additionally, Ann Masten’s Chapter on “Ordinary Magic: Resilience processes in development” should be referenced.

These additional references have been added to the introduction section of the manuscript and the text modified to reflect their inclusion (page 4).

2. All aspects of the manuscript (e.g., literature review, study design, and discussion, in particular) would benefit from a more developmentally sensitive approach. For example, why did the Australia study, cite 9, discussed on page 5 have diminishing effects, developmentally, for smoking (yet and an effect for marijuana in grade 10)? What did those authors discuss? What are the implications?

A lack of comment by cited studies restrict the ability to expand more on the developmental aspects of the resilience interventions described in the manuscript. However the introduction, study design and discussion have been amended to address the developmental trajectory of substance use and clarify the intervention aim of reducing the extent of adolescent substance use (pages 5, 6, 16).

In another Australia study (cite 23, 32), how should the reader interpret the posthoc findings? This needs to be interpreted in light of extant theory and empirical research.

Interpretation of the posthoc findings has been included in the paragraph following the description of the findings (page 5).

3. The paragraph on pages 5-6 regarding issues with intervention implementation is excellent. More of this is needed. Further, did these issues re: implementation vary by grade/age or where they more school wide?

The study does not report any differential implementation issues by grade/age and further discussion of issues with intervention implementation have been added to the introduction (page 6).

4. Why was a non-controlled design selected? Although it is a pilot study further justification is needed. Additionally, a study that provided the foundation for your work is not mentioned until the discussion (p.14).

Please see response to reviewer 1, revision 2.

The study by West et al has been correctly referenced in the introduction (page 4).

5. Please provide the consent rate for parents and the assent rate for youths.
The participation rate has been amended and is inclusive of both parent active consent and student participation (page 12).

6. Are the memorandum of understanding and school action plan unique to your study? Please discuss the importance and prevalence of these strategies.

Memorandums of understanding, or partnerships between schools and health organisations as well as school action plans are common strategies within school health promotion. With both considered to increase the likelihood of changes in school policies and practices being adhered to, and being sustained. Relevant references and additional discussion on both of these strategies has been included in the intervention description (page 9).

7. Provide details on staff training or refer reader to another manuscript that does so. In fact, the details of the intervention are scarce.

References to staff training and additional details regarding the intervention have been included in the intervention section of the manuscript (pages 8-9).

8. Findings need to be unpacked further in the results and discussion section instead of leaving the reader to decipher the tables. For instance, in the manuscript the authors do not sufficiently address that school C remained identical in their overall median resilience and protective factor scores. School C also evinced declines in substance use, despite no improvement in resilience or protective factor scores (but they remained the same). This finding warrants further examination and explication. Were there issues with fidelity at school C only?

Comments regarding the findings for school C have been added to the discussion (page 16).

9. At the bottom of page 15 in the discussion, shortcomings of previously referenced studies (cites 23, 32, 38, 46, 9, 10) are mentioned but it is unclear what aspect of those studies were related to their limitations and explicitly how the present pilot study improves upon these issues or not.

The description of how the current study improves on the limitations of previous studies has been modified as per previous responses. Addressing these limitations is the explicit focus of the study as stated in the revised aims, description of intervention, and in the discussion (page 6 and 14).

10. The limitations cited at the top of page 16 seem quite major. Despite these issues how does the present study uniquely contribute to the literature? For instance, research detailed in the introduction also demonstrates a declining trend in substance use.
The description of how the current study contributes to the literature has been modified as per previous responses. Despite the declining trend in substance use, the prevalence of adolescent substance and the resulting impact of adolescent and future adult health remains a problem. The discussion has been amended to make the contribution to the literature clearer, in particular that such an intervention with explicit adoption strategies is feasible and given positive results suggest there is merit in further investigating such an intervention due to the potential for such an approach to deliver population wide health benefits via broader dissemination (pages 14 and 19).

11. The authors acknowledge that their study design does not allow for comparison against statewide school surveys but then proceeds to make the comparison. Please avoid this comparison or provide adequate justification for this approach and a more thorough explanation of your argument.

This paragraph in the discussion has been amended to clarify the use of the statewide data as a means of indicating to the reader the broader context of temporal trends in substance in which the study and its findings are located (pages 17).

12. Isn’t it possible that the effect is greater in the present study because the schools were particularly disadvantaged to begin with?

The impact of low socio economic status could potentially have had the effect as described. Alternatively, it could be argued that modifying health risks among disadvantaged populations is more difficult, as evidenced by the greater prevalence of health risk behaviours and the lower responsiveness to interventions by this population group. The potential for either interpretation has been added to the discussion (page 17).

13. This may be beyond the scope of the manuscript however, it would substantially add to the paper: If some interventions work whereas others do not, what did your pilot study expose as the key ingredients for a successful intervention? In other words, what does your pilot study uniquely contribute to the literature? What are some caveats?

As described in the introduction, no studies have assessed the effect of a comprehensive or adequately supported resilience intervention approach. In this context, evidence is required in the first instance that such an approach is effective. In the event that rigorous research confirms this potential, subsequent studies seeking to establish the relative effectiveness and cost effectiveness of the intervention elements would be warranted. Amendments have been made to the discussion and the conclusion to reflect this (pages 14 and 19).

Minor Essential Revisions
14. Typos in references section (e.g., for the Gottfredson citation on page 22, delinquency is misspelled)

This has been corrected in the reference section of the manuscript.
Discretionary Revisions
15. Page 3, cite 50, please provide an example of “provision of information”.

An example and reference to an intervention that Thomas et al cited as information-giving curricula has been included in the introduction (page 3).