Reviewer's report

Title: Parent's alcohol use and family interaction associated with the initiation of alcohol use by the sixth graders: A preliminary study in Taiwan

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Reviewer: Katherine Shelton

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Parent's alcohol use and family interaction associated with the initiation of alcohol use by the sixth graders: A preliminary study in Taiwan

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This manuscript presents findings relating to the association between the concordance between parents alcohol use and the initiation of alcohol use in a country which has low rates of alcohol use. The work is of potential interest to those working in the area of adolescent alcohol use, particularly use in non-Western countries.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Abstract:

1. The background to the study should provide some information relating to the rationale for the aims. Why is this an important issues to address and why the focus on family interaction and not some other factor?
2. Would it be more appropriate to refer to participants instead of subjects?
3. The abstract should also define the predictor and outcome variables as opposed to listing all of the study variables.
4. The reference to fifth and sixth grade is not informative for most readers and should be replaced with the age groups sampled.
5. The conclusions are something of a leap from the study findings. It is a step too far to suggest that these findings show that parents should serve as role models for alcohol non-use or that good parent-child relationships and provision of support would encourage abstention. These conclusions seem to presume the role of social learning, when several other mechanisms may be at play.

Introduction

Paragraph 1: There are sections of text that stray beyond the focus of this study. For example, the role of education about alcohol use is not something considered here. In addition, there are several points that require greater clarity, such as the concept of ‘social transition’.
Several comments are unsubstantiated with evidence. One example appears on page 4 where it is argued that previous research has considered the effect of mother and father alcohol use separately without considering the effects of a potential ‘double whammy’ of both parents’ drinking behaviour. There are many statements like this in the introduction and I would like to see the authors be more thorough in citing research evidence to support such claims.

Method

More information should be provided about the sample including age, gender and other demographic detail about the participating families. Table 1 should also be reorganised because some variables appear twice (parental support, conflict) and it would be more appropriate to list these the demographic information before the study variables.

The measures section should include the internal consistency estimates for the scales used (conflict, violent tendencies). The description of the measure ‘violent tendencies’ is a glaring omission in this manuscript. This variable turns out to be significantly associated with the outcome measure of alcohol use. However, we don’t know whose violent tendencies it refers to, the origin of the scale, the internal consistency, the reason for including it as a predictor etc.

Data analysis: The measures section refers to the dependent variable as schoolchildren’s alcohol use. Actually, it appears to be group membership.

Results

The first section refers to percentage comparisons. The authors should conduct chi-square tests to check for differences between the users and never-users, and t-tests for the mean levels of parental support and family conflict.

On page 8, I think the odds ratios have been misinterpreted. I think the authors should check the figures here.

The sentence beginning on page 9, ‘There was no significant relationship…’ should be rephrased as, ‘A non-significant relationship was found between’.

Discussion

The opening sentence is vague: “Past research has usually compared alcohol use at different time points” is vague and should be rephrased.

I think the discussion should emphasise the health implications of early initiation of alcohol; for example, that it can lead to heavier use at a younger age. This idea could also be considered in the introduction so that the reader has a sense of why we should be considered with the influences on initiation of use in childhood and adolescence.

The introduction of a social learning theory perspective on the findings is one explanation for the results, but there are others that the authors should consider. I wonder whether it would also help to cite this perspective in the introduction.
Page 12: The points made about why parent support and not family conflict was associated with alcohol use do not make sense. Could it be a reflection of the measurement properties of these respective measures?

The view that the study makes four major contributions to this field of research strikes me as a rather strong statement to make and would prefer to see a more tempered conclusion. More generally, by page 13, the authors make several causal statements about the findings which can not be substantiated, for instance, the use of phrases like ‘influenced’ and ‘delayed’. In addition, the authors suggest that these findings support ‘prevention interventions’ with youth regarding alcohol use. Based on the findings of this study, it would perhaps make more sense to educate parents about the effects of alcohol use on children.

The limitations section suggests that these findings are only applicable to children at public schools in Northern Taiwan. This statement completely undermines the manuscript and should be revised, perhaps to state instead that the findings require replication in other low alcohol use countries.

The authors ignore the considerable literature indicating that the initiation of alcohol use in childhood and adolescence is partly heritable. Some reference should be made to this work.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct).

Writing style
There are some grammatical errors. The use of abbreviations e.g. WHO without a first reference needs correcting.

Abstract
1. What is secondary data analysis?
2. The key words are vague and should be changed.

Level of interest: An article of limited interest

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

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.