Reviewer's report

Title: Association between substance use and psychosocial characteristics among adolescents of the Seychelles

Version: 2 Date: 31 May 2011

Reviewer: Karla Wagner

Reviewer's report:

The authors were fairly responsive to the reviewers’ comments and have made substantial changes to the manuscript, including major changes in the analysis strategy. The resulting manuscript is much improved and has the potential to make a significant contribution. Just a few concerns remain:

Major Compulsory Revisions:

1. The limitations section now mentions that there might be multicollinearity problems, but I believe that the impact of multicollinearity could actually be assessed and reported, no?

2. The authors say that they have now included all students (and not excluded those with missing values, except for 15 with missing gender). The final sample size for the regression models should be reported. If all students were included in the final regressions, how are those with missing data retained? Were their values imputed? This should be described.

3. I agree with Reviewer 3 that clustering is likely a concern, and most school-based research at this point does account for this potential. While the authors may not have expected clustering to influence their findings, it probably should be dealt with, anyway. At the very least, it would make a big contribution to the school-based research literature if the authors would calculate and report the ICCs, providing justification that they were low enough not to matter.

Minor Essential Revisions:

4. All 3 reviewers raised questions about the selection of the psychosocial explanatory variables, and some of us questioned the rationale for using the individual explanatory variables rather than summary scores based on factor analysis (e.g., those indicative of depression, or the parenting items). While the authors revised the analysis to exclude the factor analysis in its entirety, and emphasize that they were seeking model parsimony and attempting to avoid redundancy, it is still not clear to me why they would use the individual items rather than a summary score. It seems that calculating a “depression symptom” index score or similar could have achieved that end, particularly given that the factor analysis had already been conducted. If the authors have a rationale for not doing that, it would be good to state it.
Discretionary Revisions:

5. The introduction now states that there are differences in the prevalence and correlates of risk behaviors between Western and African countries – it would be nice to include an explicit comparison, to allow the reader to compare. The prevalence of smoking among school-aged children in the Seychelles is reported – how does that compare to Western countries or other African countries?

Minor issues not for publication:

1. Finally, just a logistical note: I am not entirely familiar with the policies of BMC regarding the style for responses to reviewers, but I found it quite cumbersome to try and reconcile the reviewer’s comments in one document and the authors’ response in another. It would have been really helpful if the authors had summarized the reviewers’ comments in their response.

**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.