Reviewer’s report

Title: Socioeconomic status, urbanization and risk behaviours in Mexican youth: an analysis of three cross-sectional surveys

Version: 4 Date: 18 July 2011

Reviewer: Isaac Rhew

Reviewer’s report:

I appreciate the authors’ work to address reviewers’ comments. I believe the paper is much improved, but as I re-read the manuscript, a few issues remain that could use further clarification.

Major Revisions:

• Because of the nature of the intervention and the selection criteria for the intervention, it should be clear from the start that the study sample consisted of low-income or poor youth. As the authors point out in the Discussion, there is limited generalizability to higher income adolescents. Thus, it should be clear to reader even in the Abstract and Background (and perhaps even the title) that this study is focused on low-income/poor (or whatever the authors consider to be the most appropriate terminology) youth.

• I have become more puzzled by the inclusion of an interaction between marital status and gender and the lengthy discussion of the findings of these interactions in the Results and Discussion. The authors rationale for including an interaction does not seem particularly sound. They state that the “potential of being married is expected to be different for males and females.” While this is true, it is not clear why this would be differentially related to the outcomes of interest. I would imagine that the potential for having a higher level of education might be different for males and females, so why is a gender x education interaction not included? Because it was not a stated aim of the study, the inclusion of the gender-marital status interaction seems a bit tacked-on and I wonder if it was a post-hoc finding.

• I appreciate the authors’ further explanation of the timing of the survey waves by urbanicity. Honestly, I still have a hard time wrapping my head around the fact that the rural localities were sampled only in 2003, but that year was included as a covariate in the model. Though perhaps not collinear, there still remains an interesting issue that the urbanicity parameter adjusted for year is based on extrapolations. In interpreting adjusted models, we typically consider the effect of a characteristic holding another variable constant. So, we’re focused at the effect of rural vs. semi-urban vs. etc, for any given year, but rural localities are not sampled in 2001. What might help me personally is if the authors show me the distribution of the year variable across the 4 urbanicity categories as well as the parameter estimate for year from the statistical model. It might also help me to see the model not adjusted for year.

• I appreciate the added discussion of the SES x urbanicity interaction. However,
I am unclear about how it was determined whether the interaction was an important/significant one. It seems clear that there is a differential association between urbanicity and sexual risk behaviors by SES. But as I look at Table 2 and the figure, it doesn’t appear that there was much difference in the association between urbanicity and any of the alcohol/drug use items across the 4 SES strata. Yet in the Discussion, authors talk in general of how the risk behavior-urbanicity association is moderated by SES. If there is indeed moderation by SES for all outcomes, the authors should provide evidence for this. If not, the authors should be careful to limit discussion of the SES-urbanicity association to sexual risk behaviors.

• In the Results on p. 14, authors report a U-shaped trend for the impact of locality size on substance consumption across the 4 quartiles. This is not apparent to me. For alcohol and lifetime smoking, it appears that there is a higher probability of use among rural adolescents and a lower probability that was similar among adolescents from the three more urban localities.

Minor Essential Revisions:
• Please include the distribution of all covariates (i.e. include year and education) in Table 1.
• Authors should state what asterisks indicate in Table 2 in the footnotes. I assume they are indicators of the level of statistical significance, but what the actual levels are is unclear.

Discretionary Revisions:
• I could still use more discussion from the authors about their thoughts about why rural youth are more likely to engage in alcohol and cigarette use. In the Background, authors discuss earlier adoption of adult roles such as marriage among rural youth. However, in their sample, the proportion of those married among rural participants is similar, if not smaller to more urban participants. Thus, the argument about earlier adoption of adult roles does not seem to fit here. Authors also discuss poverty, but rural locality appears to be an important risk factor for alcohol and cigarette use within each strata of SES. Thus, something beyond SES of rural communities seems to be driving this.

Level of interest: An article of importance in its field

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.