

Author's response to reviews

Title: A descriptive study of youth risk behavior in urban and rural secondary school students in El Salvador

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Author's response to reviews: see over

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Editorial Board

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Dear Sir/Madam:

Thank you very much for facilitating the constructive review of our manuscript entitled **“A descriptive study of youth risk behavior among urban and rural secondary school students in El Salvador,” MS: 1497763434903756**. I am attaching the revised manuscript along with the tables. Please find below a point-by-point response to the reviewers’ concerns along with a summary of the revisions made to the manuscript.

REVIEWER 1 (Minoru Takakura)

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

1. There is a need for authors to provide further explanations of public health problems in El Salvador, regarding burdens of disease, major causes of death among adolescents, and any differences in health problems between Salvadorans and other Latin Americans.

Response: We have framed the findings of the study by including a description of the major causes of death in El Salvador (pp. 14 & 15). We have also strengthened the discussion by incorporating more comparisons of our findings with youth risk behavior prevalence from other Latin American countries (see Discussion section, pp.15-20). Many of these findings had been published recently and were not available when this project was initiated.

2. In this study, youth risk behavior measurements based on the US CDC’s Youth Risk Behavior Survey (YRBS) are comprised of five behavioral domains. The original YRBS questions include six domains of health risk behaviors. Therefore, it is necessary to give reasons why these five domains have been targeted in this survey. In addition, there is a necessity for detailed information regarding questions (e.g. reference periods, methods of dichotomizing response categories) to be given in the text.

Response: We have included a rationale for limiting the survey to the five behavioral domains (see p.8, 1st paragraph) and cited the need to include risk behaviors related to nutrition and physical activity in the limitations section (p.

- 21). We have also strengthened the description of the study measures (p. 7, under “Youth Risk Behavior Measures,” and page 8, both paragraphs.).
3. The prevalence of risk behaviors by school type and subjective economic status are not shown in the text or tables. As this is a descriptive study of risk behaviors among Salvadoran adolescents, it is necessary to describe any prevalence of risk behaviors and give all results of bivariate analyses.

Response: We explored further the calculation of the prevalence ratio thanks to the Reviewer’s question raised under point 8 below about the controversy over Zhang and Yu’s calculation. As Zhang and Yu’s method has been questioned [1] and some have questioned the use of risk ratios over odds ratios [2], we re-analyzed all results using logistic regression and prevalence odds ratios. In our re-analysis, we found that subjective economic status was significantly associated with 3 of the 19 behaviors and therefore included this variable in all analyses in order to allow comparability across five behavioral domains. Thus, the bivariate results and the adjusted results for subjective economic status have now been included in the tables. Regarding model/regular school type, we have indicated in the text that there were no statistically significant differences with any of the youth risk behaviors (see p. 9, 3rd paragraph). In addition to the lack of significance, during the data collection phase of the study we found that the distinction between model and regular schools was more in theory than in practice. Interviews with all school principals and teachers at the time of the study (from both ‘model’ and ‘regular’ schools) indicated that this program was not functioning and few differences in educational approaches could be attributed to this classification. As such, we are hesitant to expend more space within the text or tables to present these results.

4. Similarly, confidence intervals should be added for estimation of the prevalence of each risk behavior in the Results section or Tables.

Response: We have included confidence intervals for all risk behaviors in the tables (see Tables 1-5).

5. Further explanation about the school system in El Salvador is required.

Response: We have provided a brief explanation of the school system in El Salvador (p. 5, 2nd paragraph).

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

6. Information concerning the handling of missing data is needed.

Response: We have indicated that listwise deletion was used to handle missing cases. (p. 9, 3rd paragraph).

7. The following possible confounders have not been taken into account in this study: ethnicity, religion, and academic performance. The authors need to mention this possible limitation.

Response: We have cited the failure to include religion and academic performance as a limitation under this section in the discussion (p. 21). Few studies (if any) include ethnicity as a variable in research in El Salvador as this country has a very homogenous and primarily mestizo population.

8. I could not retrieve Mitchell's relrisk ado program application for Stata from <http://www.ats.ucla.edu/stat/stata/ado/analysis/> (reference 9). The authors should indicate the correct URL. In addition, according to McNutt et al. (Am J Epidemiol 2003;157:940-943), the corrected prevalence ratio proposed by Zheng and Yu is controversial. The authors should address this point.

Response: See response under point 3. I also communicated directly with UCLA, and they indicated that Mitchell's program had been removed as per the controversy indicated by the Reviewer. We very much appreciate the Reviewer's feedback regarding Zhang and Yu's calculation.

9. Do not use vertical lines in Tables.

Response: We have removed vertical lines in the Tables. Other modifications: we included subjective economic status as mentioned under point 3, reanalyzed the data with adjusted prevalence odds ratios, bolded the significant prevalence odds ratios, and removed Table 1 (see point 11 below).

Discretionary Revisions (which the author can choose to ignore)

10. The authors mention that "...calculating prevalence ratios and 95% confidence intervals using SPSS software" (page 8). What kind of programs or commands of SPSS were used?

Response: We reran the analysis in Stata 9 using logistic regression command and adjusted odds ratios.

11. I would suggest deleting Table 1. Sociodemographic characteristics of the study sample are described in the text.

Response: We have taken the Reviewer's advice and deleted Table 1. In doing so, we made minor modifications to the first paragraph of the results section to describe the sample (see p. 9).

REVIEWER 2 (Marc Mitchell)

Discretionary Revisions (which the author can choose to ignore)

1. I felt that the authors spend quite a lot of time discussing the differences within the sample groups (e.g. urban vs. rural) and not enough time commenting on both the absolute levels of risky behavior and comparisons with other groups in other countries including the US. For example, 7-10% of students had been forced to have sexual intercourse, which seems high but may not be significantly higher than the US numbers.

Response: We have included additional information on youth risk behavior prevalence from various Latin American countries in an attempt to refocus some of the discussion on the prevalence estimates found in this study as well as to provide a comparison on which to assess the magnitude of the Salvadoran risk behavior prevalence. (see pp.15-16, and various additions on p. 17 [references to Nicaragua and PACARDO study], p. 18 [references to Guatemala and Nicaragua], and p. 19 [references to Nicaragua]).

2. One type of analysis that may be interesting is to look at sub groups of students for certain questions. For example, it would appear that most females who are sexually active (7.8%) were forced to have sex (7.2%). Is this correct and if so, what does this mean?

Response: We appreciate the reviewer's suggestions and have reanalyzed this variable by focusing specifically on those who reported having had sexual intercourse. In doing so, we found a large proportion of sexually experienced female adolescents (20%) reported forced sexual intercourse and that females were significantly more likely to report forced sexual intercourse than males. We have added a paragraph to discuss this finding (see p. 17, 2nd paragraph), have made modifications to Table 5 to display these results, and have added this finding to the abstract.

3. There is considerable discussion of the data comparing the US Hispanic population and the El Salvador one. However, did the authors control for variables such as urban, age, etc.? It is not clear to me that these populations are in fact similar.

Response: As we discuss under point 1, we have attempted to enhance the discussion by including risk behavior prevalence estimates from other Latin American countries. Our intent of including risk behavior prevalence from the U.S. Hispanic population was also to provide some reference for interpreting the magnitude of the Salvadoran risk behavior prevalence. The U.S. data is nationally representative but was not based on a sampling design that could generalize across urban and rural areas. Thus, we were somewhat limited in terms of sample size to compare metropolitan status. However, in order to strengthen the comparability of these estimates, we did modify this paragraph to

specifically compare risk behavior prevalence of the U.S. Hispanic population of similar age based on U.S. YRBS data available from the CDC of the same year as our study (see p. 15, 2nd paragraph).

Other minor modification:

- We changed the word ‘among’ to ‘in’ in the title of the study.

I hope we have adequately addressed the concerns of the reviewers and that the revised manuscript is responsive to their insightful and thoughtful criticisms of the earlier draft. Again, we greatly appreciate the insightful and comprehensive review of this manuscript. Please do not hesitate to contact me with any needs for additional clarification.

Sincerely,

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encls. Revised manuscript with tables.

References

1. McNutt LA, Wu C, Xue X, Hafner JP: **Estimating the Relative Risk in Cohort Studies and Clinical Trials of Common Outcomes.** *Am J Epidemiol* 2003, 157(10):940-3.
2. Cook TD: **Up with odds ratios! A case for odds ratios when outcomes are common.** *Acad Emerg Med* 2002, 9:1430-4.