Author's response to reviews

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

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Version: 7 Date: 27 October 2011

Author's response to reviews: see over
Dear editorial board:

We are submitting a new revised version of the manuscript number 2712974524208514: “Socioeconomic status, urbanicity and risk behaviours in Mexican youth: an analysis of three cross-sectional surveys”. To our understanding, this version addresses all the points made by the reviewers. We would like to thank the reviewers for their comments, which helped to improve the manuscript. Our point-by-point responses to the comments of the two reviewers are below.

We look forward to hearing from you soon,

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Reviewer: Isaac Rhew
Reviewer’s report:
I applaud the authors’ efforts to continue to revise and improve this manuscript. I appreciate their responses to my particular comments during the last review. Indeed, many of my concerns were addressed. Despite this and the fact that I have now seen multiple versions of this manuscript, issues still remain, largely with continued lack of clarity in the sampling procedures and implications due to sampling.

I think the authors should continue to work to clarify the sampling procedures because they are so complex and they are extremely important for understanding how this study is or is not generalizable to other populations.

We understand this concern. It is not an easy task however; as our study is taking advantage by the fact that we have access to 3 different large samples of Mexican households. While the overall aim for the 3 surveys was the same, as well as the basic procedures for sampling, including all details for the 3 surveys would require a different document. We believe that the additional information added could help to better understand the general approach. For instance, we have added in the manuscript more information regarding sampling procedures in page 6, 7 and 8. Also, we have added that our results are generalizable only to population of Mexican adolescents from low-income areas (second paragraph, page 17).

Since a number of participants were excluded because of participation in the Intervention, the authors should clearly list the eligibility criteria for the Intervention in the Methods section.

The sample of our analysis is composed of both eligible and non eligible households that were not incorporated into the program yet, since these surveys were not directed exclusively to eligible households but also to non-eligible households in the same areas selected for evaluation purposes. We added a paragraph in the Methods section addressing this issue (last paragraph on page 6 and first paragraph on page 7). Our aim for using this criterion for the analysis was to avoid confusion between potential program effects and the interaction we are interested in study in this paper. In this sense, we don’t think there is a potential bias as we are comparing households / adolescents in the 3 surveys that were surveyed based on similar criteria and prior to their incorporation in the program.

The years listed for the surveys on pages 6 and 7 jump back and forth which is challenging for the reader. On page 7, the authors should find a clearer way to describe the sampling procedures. The authors should somehow make it very clear what survey they are referring to. Also, for each survey (the primarily small urban sample in 2001, the rural sample in 2003, and the urban sample in 2003), the authors should state how many households/participants were surveyed, response rates, and how many (%) were excluded due to participation in the intervention.

We have modified the text regarding sampling procedures. For instance, in this new version we present first the survey in rural areas instead of the semi-urban survey of 2001 (see page 7 and 8). We also added information regarding the total of participants surveyed and excluded due to their participation in the program (page 9, data management).

In this 2001 sample, it is not clear whether this was also primarily a low-income sample.

Yes, as mentioned, the general approach for the 3 surveys was the same, sampling areas with a high concentration of poor households according to information from the Mexican Census. We added this on page 8 (first paragraph).

In the 2003 sample, what were the criteria to define a “marginalized” area? How were intervention and non-intervention zones defined?
Marginalized areas are areas with a high concentration of low-income (poor) households. The classification into intervention and non-intervention zones was done external to our analysis, as the program defined which zones were to be incorporated early and which ones later according to their planning.

An example of the confusion, on page 7, the authors state that in 2003, the survey targeted the previous rural panel. In the previous paragraph, they discuss the 2001 survey which targeted “small urban localities”, not rural samples. We wanted to state that in 2003 the survey targeted the original evaluation sample from the rural localities. We have clarified this (page 7).

Now that it is more clear that this sample is primarily low income, the rationale to examine differential associations between urbanicity and behaviors by 4 categories of SES does not seem particularly clear to me. The authors should provide some rationale to stratify by 4 quartiles of SES in an already low-income group. Is there that much variability in SES in this low-income group to support this? Perhaps a two- or perhaps three-category group makes more sense.

We understand this concern; however as stated in the manuscript, official statistics report that half of all Mexicans are living in poverty and thus exists a high heterogeneity in socioeconomic status even among low income households.

While data for sexual behaviors may indicate a significant SES interaction, this does indeed seem difficult to generalize considering the selection criteria for this sample (p. 16). The authors should be very careful with their interpretation of this.

In this new version we have stated in the text that our results are only generalizable to population of Mexican adolescents from low-income areas (second paragraph, page 17).

To be honest, considering the number of tests conducted and the lack of a clear pattern in the data (table 2), it’s hard to know whether those seemingly different locality-sexual behavior associations by SES are true. Adjusting for SES makes sense, but to look for interactions is not particularly convincing.

We make use of an interaction term because we wanted to identify how SES could modify the effect of locality size instead of looking for the effect of locality size or SES alone. However, we have modified the text regarding the interpretation of our results, in order to make it clear that we didn’t use any particular statistical test or value to conclude that there was an interaction effect, and thus our results should be interpreted carefully (page 17, first paragraph).
Reviewer: Joanna Stewart

Reviewer’s report:

1. Abstract Methods. As I commented last review, the second to last sentence in this method is misleading. It reads as if it is classified according to the number of inhabitants in a house (residence = house) I do not believe this is what you mean. A solution would be to replace the word residence with locality.

We appreciate the Reviewer suggestion. We have modified the Abstract section to address this concern.

2. Abstract conclusions – In the results you state no association was found between locality size and sexual initiation. However in the conclusions you state the relationship of locality size to sexual behaviours is moderated by SES. This is contradictory to the results statement and should be restricted to condom use.

We have addressed this issue and modified the Abstract section.

3. Methods 2nd paragraph - A brief definition of what makes a household ‘eligible’ would help in the understanding of the sample.

Eligibility of a household is determined by their socioeconomic status. There is an administrative cut-off point for the socioeconomic index that defines eligibility. We added this information on page 6 (last paragraph).

4. Methods p10, top, clarify whether the quartiles were the population or sample quartiles.

The quartiles were created according to the distribution of the poverty score in the analytical sample. This information was added on page 10.

5. Methods last paragraph. Ai and Norton’s approach does not suggest not looking at the interaction term although certainly the method of doing so is more complex. If you are not going to report anything about the overall interaction you need to be very cautious about commenting on differences in effects in different subgroups as you have no evidence that observed differences are real. You certainly must not state they differ significantly.

We don’t completely agree with the Reviewer when affirming that we have no evidence that observed differences are real. We would like to clarify that the argument we are using for the analysis is not that the coefficient of the interaction term is useless, but that it has not a direct interpretation. The coefficient of this term is used to estimate the coefficient within each SES stratum. However we have revised the results section carefully to address this point. Any statements regarding a statistical significance for an interaction between SES and locality size have been removed. We only affirm that a statistical significance was found when describing the association between risk behaviors and locality size in each strata, since this is real enough as supported by a statistical test. Also in relation to this comment, we added on page 17 as a limitation of this study that: “We did not use any particular statistical test or value to conclude that there was an interaction effect between SES and locality size and thus our results should be interpreted carefully”.

6. Throughout the reporting on table 2 and the graph of the same data nothing but broad patterns should be commented on, in particular given you have not reported overall interactions. – ie you should not pick out scattered ‘significant’ results where there is not an overall pattern. They are most unlikely to be real and it makes the reading of the results very difficult. With no overall test reported the results should not be over interpreted.
We have revised the text. We only describe general patterns without mention any statistical significance for the interaction. We only affirm that a statistical significance was found when describing the association between risk behaviors and locality size in each strata.

7. P12 3rd paragraph – 1st sentence (referring to condom use) not correct.
We have edited the text.

8. Discussion – need to make it plain that the differences were for condom use – not sexual behaviours in general.
We have edited the Discussion section.

9. The limitations of the study still need to include reference to the fact there were different criteria for selection of households in different surveys (and hence locality types). Despite the authors refuting that this is so the methods state that
1. for rural those included were households not included (therefore probably not eligible?) in the original intervention in the areas originally selected plus others from areas later selected but not the most marginalised which were included in the first round of intervention. This second group were people in selected areas but not necessarily eligible households.
2. The small urban where households in area selected so may or may not have been in households eligible
3. The large urban sampled everyone, in areas eligible and not, although in different ratios and using a different eligibility criteria with some of the eligible households excluded as the intervention had started.

We have modified the text to clarify how selection of households and localities was done for each survey (page 6, 7 and 8). For instance, we have added in the text that: “Eligibility to the program is defined based on a socioeconomic score that is estimated using socioeconomic and demographic characteristics such as housing materials, assets, demographic structure. The program defined a cut-off point for this score, so households with a score above a cut-off point are considered eligible for the program and the others are considered non-eligible.” (page 6, last paragraph). “Surveys included both eligible households and non-eligible households from the same areas.” (page 7, second paragraph). The sample for our analysis is composed of both eligible and non-eligible households. Below we provide some clarifications concerning the points indicated by the reviewer:

1. The localities in which the program was not yet available and that were selected for the 2003 sample do tend to have fewer poor families (in terms of the concentration of poor households), but that doesn’t mean that there were not any eligible families in such localities. These additional localities were chosen using a matching procedure that allowed to identify localities similar to those selected in 1997. In the original localities, the non-incorporated households include both non-eligible households and non-incorporated but eligible households (this are in general new households).
2. In 2001 the program targeted semi-urban areas with a high concentration of poor households (according to the 2000 Census) for its expansion.
3. The complete urban sample of 2003 included households from both intervention and non-intervention zones. The classification of these zones was done according to the concentration of poor households, that is, intervention zones included higher number of poor urban households. In the intervention zones, a probabilistic, stratified and clustered sample of 149 blocks (of households) was selected. The selection of blocks from non-intervention zones was done using matching procedures to identify blocks similar to those selected from the intervention zones. Households incorporated into the program were excluded from our analysis, however the sample still includes eligible and non eligible households.
Although it is very hard to get ones head around how these differences will have affected the characteristics of those selected in the different regions, particularly as no explanation has been given as to what makes a household ‘eligible’ it will most definitely have had some effect and therefore needs to be acknowledged. It is important to clarify that we did not exclude households based on eligibility but based on incorporation, so we are not expecting a bias for this. Eligible households are those that according to program rules, are poor enough to need the program. We added some text in the Limitation section to state that different sampling techniques may have affected our results (see page 17, second paragraph).

Minor
1. Background – 2nd paragraph – second sentence needs rewording – escalation of rate of world's urban areas’ does not make sense. Do you mean the proportion of people living in urban areas?
   We have edited the text as suggested.

2. 3rd paragraph – needs editing as below
   Official statistics indicate that about 46% of Mexicans live in poverty, and 10% in extreme poverty [5]. Additional to this figure, a recent concern in the country relates to the young population: different surveys have show that about 22% of individuals between 12 and 29 years old are neither in school nor working [6].
   We have edited the text as suggested.

3. 4th paragraph, last sentence – insert ‘are’ after ‘which’
   We have edited the text as suggested.

   We have edited the sentence.

5. Methods, Sampling procedures and data collection – 2nd paragraph 2nd sentence needs editing – not ‘two moments’
   We have edited the sentence.

6. Results p12 2nd paragraph – 6th line – find, not found.
   We have edited the sentence.

7. P14 2nd paragraph – provide (not provides)
   We have edited the sentence.

8. P15 2nd para - rapid, not rapidly
   We have edited the sentence.

9. P16 2nd paragraph ‘are usually reinforced each other’ needs rewording.
   We have edited the sentence.