Author’s response to reviews

Title: Identifying High Risk Subgroups of MSM: A Latent Class Analysis using Two Samples

Authors:

M. Kumi Smith (kumi.smith@unc.edu)
Gabriella Stein (ghstein123@gmail.com)
Weibin Cheng (bin_cheng817@163.com)
William Miller (miller.8332@osu.edu)
Joseph Tucker (jdtucker@unc.edu)

Version: 1 Date: 28 Aug 2017

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RESPONSE TO REVIEWER COMMENTS

Reviewer(s)’ Comments to Author:
Reviewer: 1

Comments to the Author
This a well written paper that uses LCA with two different samples drawn from a nationwide survey and a survey at only one province in China. However, this reviewer has some major concerns that need to be addressed before it is suitable for publication:

1) My major critique of the paper is that it reads in the abstract and introduction as though the main aim is to assess the ability of LCA to properly identify risk profiles, but actually the paper is using LCA to assess the validity of certain aspects of the research design. A revision of the paper will need to make this point crystal clear to the reader.

We feel that statements included in the Abstract and Introduction make clear the main objective of our analysis (text quoted below). However we agree with the reviewer that more text could be added to underscore the central objective; as such we added the following sentence to the beginning of the Discussion section. “This study investigated the generalizability of latent class structures identified using LCA by conducting identical analyses on two distinct samples. Differences in the inferred population structures from each sample highlight features of sample design that affect robustness of
Abstract: “We compare LCA performed on two online samples of HIV negative Chinese men who have sex with men (MSM) to detect more generalizable latent class structures and to assess the extent to which sampling considerations impact the validity of LCA results.”

Introduction: “Contextualizing the public health insights informed by LCAs must take into account the generalizability of subgroup structures identified when using samples with known biases. To investigate the robustness of group structures identified by LCA models, we conducted the same model analysis on two distinct online samples of Chinese MSM: a survey conducted in a single locale and a nationwide survey.”

2) I have some concerns, generally, about comparing a survey that was conducted online and one that was conducted face to face in the clinic and required testing even without the LCA, we would expect that these samples are going to be different.

We share the reviewers instinct for caution when comparing two datasets sampled using such different recruitment methods. However, rather than a direct comparison of these two dataset, we were rather conducting 2 parallel analyses on each sample to test the robustness of conclusions drawn from LCA’s. That is, given the presumed differences between the two samples, if an LCA conducted on each yielded identical latent class structures, this would have served as proof of the robustness of the underlying structure. As the analysis turned out, the LCA results varied slightly by dataset. Such results in turn provided us with helpful insights as to ways in which characteristics of each sample (e.g. more high risk men self-selecting into the clinic-based Guangzhou survey, or a broader swath of the MSM community—including lower risk individuals—self-selecting into the national online survey) may have influenced the results that were observed.

To further clarify our intent in analyzing both surveys, we added an additional phrase to second to last sentence of our Introduction paragraph to read, “The goal of the comparison in LCA results across two separate surveys with distinct sampling recruitment methods is to gain insights into the extent to which inferences may be influenced by study design, recruitment methods, or phrasing of questions.”

3) In addition to #2 above, one of the major conclusions as stated in the discussion is that the online sample is more representative on China than the one province only survey, but this is something we already know. A survey conducted in one particular place should never be considered representative of the whole.

We agree with the reviewer that the conclusions of this study should better match our stated interest in how sample features affect LCA inferences. We have therefore restated the major conclusions in the Discussion section to read, “1) the presence of a sizable and distinctly lower risk class in the nationwide online sample likely explains the difference in the observed latent class structures between the LCA’s conducted on the nationwide online survey and the Guangzhou sentinel surveillance survey, and 2) a common features of both LCA results was the presence of a small, highest risk group in each sample defined largely by their tendency to endorse group sex.”

4) The authors state in the paper that motivations to test may be one reason why the in person survey didn't have lower risk participants however, the convenience of the online survey probably also plays a role.
We agree with the author that convenience was certainly a factor in the make-up of the nationwide online survey. However, our concern in this analysis is regarding sample features that would generate sampling bias. That is, a feature that would make MSM more or less likely to participate depending on their risk class. Because convenience of the online survey is equally convenient for all MSM, we would only expect it to affect the response rate (i.e. overall larger sample size) but not to alter the proportion of high versus low risk men.

5) The first two paragraphs of the discussion are a rehashing of the results the space could instead be used to dive more deeply into the implications of the findings.

The reason for what appeared to be “rehashing” of results was due to an ill-placed breaker for the subsections of the manuscript. To streamline the distinct presentation of results, discussion, and conclusions (and to better reflect BMC Infectious Disease formatting guidelines), we reformatted the Results, Discussion and Conclusions sections.

6) The end of the paper should include a more definitive summary of the paper the final sentence of the paper, as it is currently written, includes a recruitment suggestions for future studies rather than a wrap-up of the papers implications.

We agree that the Conclusions section would benefit from a more definite summary. We added the following to the final paragraph of the paper: “In summary, by combining results from two simultaneous LCA’s conducted on distinct samples of Chinese MSM, this analysis provided more robust insights than would have been possible from a single LCA. Results presented here may serve as a template for future LCA’s but also catalyze greater reflection among public health researchers regarding ways to strengthen our methodological approaches to mapping and characterizing HIV risk.”

Reviewer 2

Comments to the Author

1. Many findings are listed in the conclusion. These should go into results. It would also be helpful for the reader if you labelled the latent classes with actual names.

As noted for reviewer 1, comment 5, we have reformatted the subsections per BMC Infectious Diseases guidelines: Introduction, Methods, Results, Discussion, and Conclusions. We also renamed the latent classes with the following names for better clarity: The Nationwide Highest Risk Class, The Nationwide Lowest Risk Class, The Nationwide Moderate Risk Class, The Guangzhou Higher Risk Class, and The Guangzhou Lower Risk Class.

2. "Interacting" suggests you've included an interaction term. If these factors were simply found to be independently associated with the outcome, please modify language to reflect that.

We agree that the term “interaction” could be conflated with its typical meaning in statistical models. For this reason we rephrased relevant portions to use the term “interplay” instead.
Methods:
1. LCA analyses are well described. But please also include a description of descriptive, bivariable, and multivariable analyses.

We have added the term “univariable” to our description of the regression analysis to read, “We then used binomial and multinomial logistic regression to assess univariable associations between class assignment and odds of key factors unique to each dataset.” Our descriptive table simply shows subgroup distributions with 95% CI (a description of the CI calculation is provided in the table footnotes), the methods of which are relatively self-evident.

Results:
1. Associations appear to be unadjusted. Did you conduct a multiple adjusted model to identify independent predictors of latent class membership? If so, OR should read AOR. If not, this should be made clearer and justification provided.

As addressed above, we conducted only univariable regression analyses in order to assess associations with key variables of interest. Since our goal was to assess associations—rather than to conduct predictive or etiologic modeling—multivariable adjustment would be unnecessary. Our figure legends indicate the depicted OR’s are from univariable models; we would prefer to avoid referring to these OR’s as “unadjusted” as this term implies that we intend to later present adjusted OR’s or that adjustment played any role in our model choice (it did not).

2. Also, results need to be described in the correct way. E.g., You write: members of highest risk class had a greater odds of identifying as nonmale (odds ratio [OR]: 4.01, 95% CI,1.3012.36). The correct interpretation is that: nonmale participants (relative to males) were 4 times more likely to belong to the "highest risk" latent classes (versus "lowest risk" latent class). Your dependent variable is latent class. In other words, you are identifying predictors of latent class membership.

We thank the reviewer for pointing out that inclusion mention of the referent group adds clarity, but humbly point out that our presentation of OR results is equally accurate and easier for readers to interpret. Where relevant, we added mention of the referent group to clarify the groups being compared in the interpretation of each OR value.

Conclusion:
1. I think this section should be called "Discussion" and is missing final concluding remarks.

As noted for reviewer 1, comment 5. We have reformatted the subsections, so that the relevant section in question now falls under the Discussion section. Concluding remarks were added, as noted in comment 5 for reviewer 1.

2. Also much of the text reexplains the results. What is needed here is more contextualizing of results within the current literature. For example, how do your findings around gender fluidity align or depart from what’s previously known? Similar comment for other key factors (e.g., forced sex, closeted).

We have better labeled the sections to avoid restating the results.