Reviewer's report

**Title:** Party package travels on alcohol use and related problems in a holiday resort a a mixed methods study

**Version:** 2 **Date:** 29 May 2008

**Reviewer:** Toben Nelson

**Reviewer's report:**

**Major Compulsory Revisions**

More clarity is needed in the description of the analysis and the presentation of the results. I suspect the authors have made incorrect conclusions based on the analysis that was conducted. The progression of the model building process is obfuscated in the presentation of the results. I suggest the authors revise both the Methods and Results section to provide more transparency in the analysis.

Also, it was not clear to me why the likelihood ratio test had 8 degrees of freedom. This suggests to me that all the variables available were simply added to a larger model and compared to a null model. Please clarify. I recommend a model building process of adding and testing individual co-variates so the impact of each can be understood. Also, the authors should clarify that these comparisons were made using nested models.

Of greater overall concern, however, is the model specification for the two major analyses presented. In particular, it is not clear to me why the variable attracted by party reputation is identified as a confounder in the analyses. This strikes me conceptually as more of a cognitive mediator that could perhaps be operationalized as an expectancy construct. The authors should clarify this or defend (theoretically and empirically) why it is being considered as a confounder based on the accepted definition of the term. Similarly, it is not clear why the analyses on getting into fights included the heavy drinking variable. Again, this is probably on the causal path, not a confounder. In other words, the party package tourists were more likely to get into fights because they were drinking more heavily. I suggest testing this as a mediator, or leaving it out of the model.

The apparent misspecification of variables in the model highlights the lack of an explicit theoretical basis for the study. The authors should identify what they used for the theoretical framework of the study.

The combination of these issues has important implications for the conclusions the authors draw. I don’t think the following statement in the Discussion section is at all accurate “... but there is no compelling evidence of higher short-term harm associated with drinking among party package travelers.” I suspect the authors have covered up the evidence by misspecifying their statistical models. At least showing the progression of model building would help. The appropriate modeling of the variables, within a useful theoretical context, should then mitigate the
concerns about measurement precision raised in the Discussion section. It also has implications for the harm reduction approaches advocated by the authors.

The authors note that ordinal logistic regression models were conducted, but then refer to analysis on drinking 12 or more units per day. Is this an ordinal logistic or a logistic regression? Can the authors report on whether the data met the assumptions for an ordinal logistic regression? Some statistical packages provide a test of this. The authors should note which package they used to conduct the analyses.

How many different travel agencies were involved in this study? I suggest that the analyses be conducted using hierarchical models, with individuals nested within travel agencies.

Minor Essential Revisions

An important limitation of this exploratory study is the design and the comparisons that can be made (and those that cannot be made) as a result. The primary comparison in the survey portion of the study is between tourists who use party package travel agencies compared with those who do not. This is a reasonable comparison, particularly given the exploratory nature of the study, but it may not be the most interesting finding of this work. I recommend that the authors clearly acknowledge the limits of inference in the Discussion. Related to these design limitations, I suggest comparing the characteristics of the samples in the party package and the traditional groups with the known characteristics of young adults of the same age who reside in Denmark.

The authors may also want to consider these limitations in the conclusions drawn. Perhaps it is not how the travelers got there that matters for heavy drinking and related consequences, but the pervasive environment of heavy drinking the greets travelers who choose to go on these trips once they arrive.

I suggest that the authors include more detail about the context in which these trips occur. Who goes on them? When? How normative is this behavior? What is known about the industry that facilitates these trips? Can the authors provide additional information about Sunny Beach? If possible, I suggest providing some description of the timeline associated with the emergence of the party package tours, and perhaps a note about whether travel to this holiday destination is increasing, and whether that appeared to be associated with the emergence of the party package tours and their marketing. Is there a particular time of year when these trips are usually taken? In the US, high school and college students tend to take these trips during Spring Break. Is something similar happening with Danish youth? I suggest making this explicit in the Introduction. Is there any information about the costs (and benefits) for Sunny Beach can share? At minimum I suggest the authors reference some of the work on college Spring Break travel in the US.

Could the authors translate the quote at the end of the first paragraph of the Results section?
Discretionary Revisions

I suggest the authors specify that the consequence of the policy of the party tours is to take away wristbands for illicit drug use. Did any study participants have their wristbands taken away?

It is interesting that heavy drinkers, those who might be more predisposed to or have strong expectations about heavy drinking, were more likely to travel with the party packages. Could the authors say more about this?

The conclusion reached in the abstract does not appear to have any basis in the findings of the study. These issues are addressed in the Discussion section, but they should draw upon study findings. Could the authors describe the qualitative findings more clearly to set up the recommendations about harm reduction? In my view these are very important components of the article, but I would like to see them based on something, even if it is simply field observation.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.