Author's response to reviews

Title: Socio-demographic correlates of six indicators of alcohol consumption: Survey findings of students across seven universities in England, Wales and Northern Ireland.

Authors:

Walid El Ansari Prof (walidansari@glos.ac.uk)
Rene Sebena Dr (rene.sebena@upis.sk)
Christiane Stock Dr (cstock@health.sdu.dk)

Version: 2 Date: 9 September 2013

Author's response to reviews: see over
Dear Prof Herman Van Oyen, MD, DrPH and Olivier Bruyère, PhD
Editors-in-Chief

The authors thank the editors and reviewers for their valuable and constructive comments. Revisions and changes have been undertaken to the satisfaction of the reviewers’ comments. The points in bold below correspond with the comments made by the reviewers.

We hope that we have adequately dealt with the reviewers’ comments and that the manuscript can now be accepted for publication.

We highlight in our revised submission in red colour the changes/ additions that were undertaken.

Best regards
Walid El Ansari
Professor

**Editor’s comment**
Titles of the tables should at least include information related to the time and place and study population

• **We thank the editor for the comment. We have now corrected and amended this across the Tables**

**Reviewer: Susanna Geidne**
General comments
In general I think this is an interesting subject with an interesting target group. University students/young adults are a risk group regarding alcohol consumption and more studies and interventions should be directed at this group. The paper is overall well written and with adequate international comparisons although there are some lack of national comparisons. Some of the comments I specify below has to do with the variable age, which I miss both in the analysis and the interpretations of the findings.

• **We thank the reviewer for the comment.**

Specific comments

□ Major compulsory revisions
1. The concepts university, college and undergraduate is used side by side, to a foreign reader unfamiliar with the British school system it is hard to know if it is the same type of school or if the students in them are of the same age. A clarifying sentence on the differences or if it is the same and what ages the students are in when they go to university in Great Britain would be good.

• We have now amended this. We have added some sentences to clarify this (p. 7 last few lines before the heading ‘Measures’)

2. Do the universities in this study have a specific type of educational programs that have a high proportion of women? It also seems like the majority of the students have low educated parents, does it have to do with the type of education or the geographical locations of the universities. If so, can this fact affect the students’ alcohol consumption? This should be clarified either in the section about the sample or discussed later.

• We have now amended this. We have added a section to clarify this gender composition, along with its reference

The latest statistics released by the University and Colleges Admissions Service (Ucas) revealed a 22,000 drop in the number of male students enrolling at university. This meant that last autumn women were a third more likely to start a degree than their male counterparts, despite the fact that there are actually more young men than women in the UK. (p. 10, para 2)

3. In your sample of students the age variable has quite high standard deviations and it looks like the samples sometimes include students under the age of 18. If that is the case, it should be emphasized because if the students are under 18 they are not allowed to buy/drink alcohol? Or what is the law in Great Britain?

• In our sample the minimum age is 18, corresponding with the minimum age of first year university students. The high standard deviations are result of that more then 21.2 % students in our sample were 30 years old or more.

4. Have you tried to include the age variable in the analysis in table 2 and 3? What age the students are when they start university could affect their alcohol consumption. This variable should be considered in the analysis and in the interpretations.

• We thank the reviewer for the comment. Yes, we first ran all the analyses with age included in the multivariate logistic regression models, but the results indicated that age did not play a role in any of the alcohol consumption indicators. Then, we looked at study year and we found some differences across the year of study with respect to long duration of drinking and large amount of drinking. So we decided to use, in the final analyses, only the year of study and not to include the age (also due to collinearity as these two variables are highly correlated).

Minor essential revisions
5. On p. 8, first paragraph you have a question from the survey about ‘alcoholic drinks’. Are ‘alcoholic drinks’ defined in any way in the questionnaire (type of alcohol or size)?

• We thank the reviewer for the comment. We have now added the description to the methods section. A “drink” is defined as a glass of wine (ca 15 cl), a bottle or can of beer (ca 50 cl), a shot glass of spirits (ca 5 cl) or a mixed drink.

6. On p. 11, second paragraph, line 13, on p. 13, fourth paragraph, line 3 and on p. 13, paragraph 5, line 4 you refer to the females but that result is not shown in the tables are they?

• We thank the reviewer for the comment. It was our mistake. We have now amended this and the text is now correct: “As for the frequency of heavy episodic drinking, Gloucestershire university students reported significantly more heavy episodic drinking compared to Bath Spa students (p. 11, Table 3, Section D)”

7. On p.11, second paragraph, line 4, it says Gloustershire and Swansea students. In table 3 it says that Chester and Swansea students were more likely to engage in long durations of drinking.

• We thank the reviewer for the comment. This was our mistake, and we have now amended this and the text is now correct: “Chester and Swansea students were more likely to engage in long durations of drinking compared to the Bath Spa counterparts…”

8. On p.12, first paragraph, last sentence the perceived income insufficiency was significantly associated to all of the indicators according to table 3, but there are just 3 mentioned here.

• We thank the reviewer for the comment. This was our mistake, and we have now amended this and the text is now correct: “However, perceived income insufficiency was significantly associated with all alcohol consumption indicators.” (p. 12, para 1). We have also checked the discussion to see if there was any inconsistency but it was ok.

9. On p.12, second paragraph, first line. ‘With respect to educational variables’ could be interpreted as if there were some other educational variables besides the ones you have. But that could be due to my not so perfect English.

• We have now amended this. In order to make this sentence clearer, we specified it: “With respect to the two educational variables under examination…” (p. 12, para 2).

10. On p.13, paragraph 4, line 5 you write ‘young university students’, how old are they then, relating to my question in paragraph 1.

• We have now amended this. We have now deleted the word “young” (p. 13, para 5).
11. In the Discussion, second paragraph you write that 59 % of the students report heavy episodic drinking and it is also said in table 1. Although in table 2 the numbers for females are 64,6 % and for males 76,4 %. How can this be explained?

• We thank the reviewer for the comment. This was our mistake, and we have now amended this and the text is now correct. We made an error during rewriting the results from the spss output. The correct percentage for the whole sample with respect to heavy episodic drinking is 67.2%. The percentage for separate genders is correct. In addition, we also checked the percentage of the other alcohol consumption variables, and everything other was correct. (Table 1, p. 19) and also in discussion (p. 12 para 2 under heading ‘Discussion’).

Assessment of the work according to the journal’s points
1. Is the question posed by the authors new and well defined? The research questions are well defined and the conclusions are clear. If the results are new is hard to say due to lack of national comparisons in the study.
5. Are the discussion and conclusions well balanced and adequately supported by the data? I miss the variable age in the analysis and the interpretations, otherwise ok.

• We have now amended this according to the points above

Adtiional Changes

Other changes
• We have inserted and acknowledgment to thank the anonymous reviewers for their constructive comments (p. 16)

Reviewer: Anton Kunst

Major Compulsory revisions

- The Discussion section should focus more on the particular contribution that this paper makes to the literature. The end of the Introduction section argues that the value of this paper as compared to other studies is in three points. Two of these are elaborated in detail: the assessment of different measure of alcohol use, and the analyses of different universities within the same country. I would therefore expect the authors to reflect on these two points in the Discussion. What similarities and dissimilarities did they observed for the different alcohol measures, and what lessons do they draw from this (both for substantively and methodologically)? To what extent are the results similar for different universities, and what does this learn us about the determinants of alcohol use among university students? I think that these are the key questions that are to be addressed in the Discussion in order to contribute to the international literature.
• We have now amended this. We have now added a section to the discussion, in which we specifically addressed the different alcohol indicators and the differences between universities (pp. 14-15)

Minor Compulsory revisions

- The text of the results section tends to focus on positive or significant results. I think that it would do more justice to the results to report on the general patterns that are observed, thereby including both the ‘positive’ and the ‘negative’ results.

• We have now amended this. We have now added the negative findings to the results section when reporting findings from table 3 (p. 11-12).

- At the end of Results, when authors report on the statistically significant interactions with gender, they should mention that total number of interactions that were tested. This will inform us what proportion of all tests was not found to be significant.

• We have now amended this. We have now added this information at the end of the result section (p.12).

- For a paper that is interested in socio-demographic determinants of alcohol use, the recommendations at the end of the paper are very little focussed on such determinants. Instead, the authors recommend to “... educate student groups..” and “..inform student groups ..”. In addition of this focus on health education, I would expect them to consider a broader set of policy options, including environmental factors and alcohol supply policies.

• We have now amended this. We have now added such policy options to the conclusions (pp. 15-16).

Discretionary Revisions

- I recommend the author to discuss other studies in less detail, and to rely more on previous reviews. A considerable part of both introduction and discussion is filled with details from other studies. This distracts from the line of argument to be developed for this particular paper.

• We thank the reviewer for the comment. We acknowledge this advice and deleted some superfluous detail information from both the introduction (page 3 last para) and the discussion. However some information on the study population is relevant to provide in order to show whether a given study that is cited is adequate to compare our results with or otherwise.

- I found it remarkable to note that, even though many socio-demographic determinants did have some relationships with alcohol use, alcohol use was high among ALL socio-demographic categories. Similarly, it was high in all universities,
with no university consistently and substantially performing better. The authors may check this in more detail, and reflect on the results.

- We have now amended this. We have now discussed such findings in the new section in the discussion (page 14, last para to top of p. 15)

- I was surprised that no alcohol measure was included on the total number of drinks per day or per week. This is an alcohol measure commonly used in surveys among adults. The authors may need to explain to some readers why such a measure was not included.

  - We have now amended this. We have now explained this lack in the limitations section of the discussion (pp. 15-16).

**Other changes**

- We have now added a note in the acknowledgement section to thank the reviewers. ‘We also thank the anonymous reviewers for their constructive comments that helped strengthen the article’.