Reviewer's report

Title: Alcohol related factors may be more associated with under-reporting of alcohol consumption than socio-demographic factors: evidence from a mixed-methods study

Version: 2
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Reviewer: Jennie Connor

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Major Compulsory Revisions

1. Title.
Reading the manuscript suggests that it is the dimensions of drinking pattern rather than alcohol per se that is being investigated here and therefore I suggest replacing “Alcohol-related factors” with something that reflects this, perhaps, “Drinking pattern”. Also in the title, “more associated” does not have a specific meaning, and so if this is a valid claim it should say “more strongly associated”.

2. Abstract.
Response fractions need to be included in the abstract. I suggest, since this is a secondary analysis, that the survey response and the diary response should be added to the Methods.
The first sentence of the Results reports a difference but also needs to indicate the direction of the difference, if not the magnitude.
In the final sentence of the Results “drinking more than expected” is not interpretable without having read the paper. Perhaps, “linked to more drinking being reported in the diary than the retrospective interview”.

3. The paper does not convince me of the assertion made in the Background about the gap between sales and survey consumption, thus:
‘This discrepancy is not explained by consumption that is not captured by surveys (e.g. non-response or sampling frame issues) or alcohol that is bought but not drunk (e.g. used in cooking), because these factors are likely to be outweighed by consumption that is not captured in the sales statistics (e.g. legal and illegal imports, see Table 1 in [9]).
The authors need to justify this clearly, as much of their interpretation depends on it. I was not surprised at the gap, but would have expected it to be caused by a combination of sampling frame issues (perhaps), survey response (definitely) and under-reporting (to be investigated in this paper) I don’t expect to have to go and find a paper and look at a table in case that table convinces me that non-response of 34% is not going to affect consumption estimates. The authors need to make this case, or remove this assertion. Part of this case is to explain how they compared selection biases on consumption in surveys, with
consumption not captured in sales statistics. I can not see an obvious connection between these two measures. Another part is to explain what method they used to quantify the effect of selection bias on consumption in this survey. Have they ascertained that there is no association of response with drinking pattern or with other determinants of under-reporting? There is no reference here to the literature on non-response biases in alcohol survey research, and this is a weakness of the paper. Why does it not matter that it is an unrepresentative sample?

The issue of response level arises again with the approximately 70% response to the diary component of the study. Implications of this are not discussed, except to say there was little difference in CAPI consumption. However, these participants are likely to be different from the ones who did not comply with the request to keep the diary and the implications of this should be considered.

4. Methods, end of 2nd paragraph: The authors need to expand on the “drinking diary weight”. The weight (however it is computed) does not “make the sample representative of the English population”. It adjusts the estimates based on a group of variables that are known in the respondents and in the general population, usually sociodemographic characteristics. The reader needs to know what these are so that they can make a judgment about whether this is likely to have increased or decreased bias in the variables of most interest, given the response level and therefore the scope for change due to weighting.

5. The investigators restricted their comparison of CAPI and diary to participants who drank at least one day in the CAPI week and at least one day in the diary week.

The potential bias from this selection has not been fully discussed. For example, anyone who reported 3 drinking days on the CAPI but no days on the diary were excluded, whereas those who went from 4 on CAPI to 7 in the diary were included. This is an extreme example, and it is more likely to affect lower frequency drinkers, but the highly variable drinkers are of particular interest to these authors. In the second paragraph of limitations, the authors’ comment on this only mentions infrequent drinkers.

It is not clear why the analysis was done this way, and it would be helpful if a rationale was given. Since they were selected on number of days drinking this could affect the difference in number of days and possibly the weekly consumption but not the heaviest occasions.

It should also be mentioned (along with the comment on how different the results are from the Canadian study) that this study wasn’t designed to detect differences among people who don’t drink every week.

6. The final paragraph of the conclusion suggests it is “necessary to understand the effect of THIS on other self-reported health behaviours as well…” but it is not clear what is meant by “THIS”, so it should be clarified. This recent paper that discusses a range of health behaviours in the context of survey non-response and references some non-response literature might be of interest:

Minor Essential Revisions
There are a number of typographical and other minor errors to be corrected.

i. Abstract Methods last line, not “heaviest drinking day” but “volume consumed on heaviest drinking day”

ii. Abstract Results 1st Line: “was” needs to be deleted

iii. UK alcohol units are not used internationally and need to be defined.

iv. Intro: end of 1st paragraph: There is a cut and paste error in the sentence referenced to [10].

v. In the section “Quantitative substudy” of the Methods, the second paragraph begins “Multivariate linear regression investigated…” and the first sentence of the section “Multivariable analysis” in the Results begins “Multiple linear regression explored demographic…”

The author clearly understands that that MLR is a tool that is used to model associations between variables of interest while controlling for potential confounders. I would like to see this expressed more appropriately, so that it is clear to the reader what the investigator’s intentions were in using their statistical software, e.g. associations between x and y were modeled used MLR in order to control for the potential confounding effects of z1, z2, etc.

vi. The first quote in the qualitative results appears to be an example of social desirability bias. Is there a reason for not identifying it this way?

vii. In the final paragraph of the Discussion the authors say “responses may be affected by recall bias” when they seem to mean poor recall. Recall bias has a very specific meaning, which usually restricts its use to case-control studies.

viii. Some colloquial language is used that may not be clear to an international audience.

In the last paragraph of the Methods section “as a thank-you”
2nd paragraph of “strengths” in the Discussion: “a new take”

Discretionary Revisions

I suggest that the authors drop the use of “risk factors” to describe variables associated with the outcomes of interest. Although I wouldn’t argue this is unacceptable usage, it unnecessarily alludes to a causal relationship when no inference can be made. “Factors associated with…” is easily understood, as in the title.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests