Author's response to reviews

Title: Drinking pattern is more strongly associated with under-reporting of alcohol consumption than socio-demographic factors: evidence from a mixed-methods study

Authors:

Sadie Boniface (s.boniface@ucl.ac.uk)
James Kneale (j.kneale@ucl.ac.uk)
Nicola Shelton (n.shelton@ucl.ac.uk)

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Author's response to reviews: see over
We thank the editor and the reviewers for their helpful comments. We have addressed these in the manuscript and below and hope that our manuscript is suitable for publication based on these changes.

Dear Dr. Boniface,

Your manuscript has now been peer reviewed and the comments are accessible in PDF format from the links below. Do let us know if you have any problems opening the files.

Editor’s comment:

“This manuscript deals with the problem of under-reporting in alcohol, and low reliability of self-reported use of alcohol. I found the manuscript very important since self-reports are very often used in measuring use of alcohol and drugs. Consequently, it is very important to be aware of factors associated with under-reporting. As an associate editor I have the advantage to be able to prioritise among the comments of reviewers. Since the degree of under-reporting is assessed by the difference between a diary, with notes assumed to be made every day, and an computerized interview of the consumption during a certain week, it is important to know about how the difference was calculated and on what scale the difference was expressed.

Have rephrased the relevant sentence in the methods section to say: ‘The difference between the diary and CAPI measures of drinking were calculated for three outcomes (Table 1) by subtracting the CAPI measures from those of the diary’.

It is also important for me to understand why the responses on the “drinking diary” were weighted and how. I agree with the reviewer that a weight doesn’t necessarily make the sample more similar to the population.
We have consulted a statistician who is an expert on HSE methodology added more detail on this in the quantitative section of the methods.
The reviewers suggested more revisions”

Reviewer: Jennie Connor

Reviewer’s report:

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”.
We agree with this comment and have changed the title to Drinking pattern is more strongly associated with under-reporting of alcohol consumption than socio-demographic factors: evidence from a mixed-methods study

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.
done
The first sentence of the Results reports a difference but also needs to indicate the direction of the difference, if not the magnitude.
done
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”.

done

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?

We have re-written this paragraph, removing that particular assertion which we feel there is not space to justify in the manuscript (it is described in detail in the lead author’s PhD thesis as it forms a conceptual basis for looking into under-reporting as an important issue).

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.

We have added a sentence to this effect in the methods section and a paragraph in the discussion. In the discussion we cite Meiklejohn’s paper on response rates and suggest the continuum of resistance model as an alternative to population weighting.

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.

We have consulted with a HSE statistician and added more detail on the HSE interview and diary weights, and added that non-response bias may have influenced our findings.

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.

Our rationale for only including people who drank in both weeks was that this would bias the results in terms of making infrequent drinkers appear disproportionately inaccurate in their reporting. We have added sentences to this effect in the Methods section and hope that this explanation is satisfactory.

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.

done

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:

Cited relevant findings and clarified this paragraph

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”
done

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

iii. UK alcohol units are not used internationally and need to be defined.
In footnote on p3

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

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.

rephrased, thank you
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?
No reason was just an omission – now mentions social desirability

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.
Amended

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”

Both amended

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.

We agree, especially important to make this clear as this is a mixed method paper. Thank you.

Reviewer: Katherine Brown

Reviewer's report:

Minor essential revisions: There are several typos throughout the document (including in the abstract). A final proof read is needed before submitting for publication.

This has been done