Author’s response to reviews

Title: An exploration of the dynamic longitudinal relationship between mental health and alcohol consumption: a prospective cohort study

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Version: 2
Date: 21 March 2014

Author’s response to reviews: see over
21st March 2014

Dear Dr Denyer,

RE: “An exploration of the dynamic longitudinal relationship between mental health and alcohol consumption: a prospective cohort study” (MS: 4944215221198171)

We are pleased to be able to submit a revised version of our manuscript for further consideration by BMC Medicine. We appreciate the constructive criticisms put forward by both reviewers and have responded to each of their concerns as outlined below.

Response to reviewer one (Professor Tim Stockwell):

We would like to begin by thanking you for your kind comments on our study design and research question. We were sorry that you found the statistical descriptions a little obtuse at times and have made some clarifications in the relevant sections to try and rectify this. In terms of specific issues you raise:

1. Implications of differential stability over time in key measures

   *This both an interesting and important observation. We have provided a paragraph devoted to this question in the revised discussion section. Specifically, we have described the issue which you raise and discussed it with reference to other points made in your review (such as #3 and #5 below). We hope that you find our discussion of this issue satisfactory.*

2. […] qualify your conclusions in the abstract and final section of the text that unequivocally mental-health drives alcohol consumption rather than vice versa. […] you should also note that different temporal dynamics/relationships may apply for different subsets of adverse mental health symptoms

   *We agree. We have revised our conclusions in both the abstract and final section of the manuscript – providing a toned down interpretation of our findings in the context of the current study/sample (with greater emphasis on the specific characteristics of the sample in the revised final section). Furthermore, we note the possibility of two dynamic systems influencing alcohol intake and mental health pre- and post-clinical disorder in the revised abstract as suggested. We also note that it is possible that different relationships may exist for different mental health components.*

3. You should indicate that the SF-36 applies to a four-week i.e. longer period than the last week diary for alcohol consumption

   *We have added a line to the methodology section stating that the SF-36 is related to symptoms experienced in the previous four weeks. We have also added a sentence to the discussion section which draws attention to this limitation (the discrepancy between the window of assessment for alcohol consumption and mental health; overlapping with points #1 and 5).*
4. In my opinion the key Table 2 would be easier to follow if it was divided into four, 1 for each model

We have revised the presentation of our findings based on the suggestion put forward by Professor Ferrer (point #3, below). We hope that you agree that this is an improvement on the original large table included in the first version of our manuscript.

5. There are older literatures addressing these questions using different research designs which I believe shed light and should not be forgotten.

We have included additional examples of the relationship between alcohol consumption and mood using the study designs that you outlined. We have contrasted the findings from some of these studies with those of our own to reinforce the issue that you raised regarding timing/window of exposure.

Response to reviewer two (Professor Emilo Ferrer):

Thank you for your compliments on the methodological quality of our study. In response to particular topics you raised in your review:

1. First, the introduction is rather brief and lacks a theoretical framework from which hypotheses can be derived. The authors mention that the purpose of the paper is to compare competing theoretical models but, again, these are not described in the manuscript. It would be important to articulate some theoretical model to set the stage of the study and define specific hypotheses. This is also the case with the covariates. The possibilities that the authors describe regarding covariates are all reasonable, but it would make sense to articulate hypotheses from theory.

   We have expanded the introduction section considerably; this includes providing descriptions of the competing theoretical models and the framework/plausible mechanisms from which they were derived. In the interest of keeping the article concise, we were reluctant to expand further on the theoretical framework for each covariate; the revised introduction section is already somewhat large in comparison to other articles published in BMC Medicine. We also do not provide specific hypotheses (but allude to them on lines 7-9 on page five of the manuscript) to remain in keeping with the style of other articles published by BMC Medicine. However, if the handling Editor feels that it is necessary then we will happily develop the theoretical basis for the covariates in greater detail as well as define specific hypotheses.

2. Participants who had not consumed alcohol in the year before baseline or those with missing values for either alcohol consumption or mental health variables at baseline were excluded from the analytic sample. One can argue that if these two processes are linked, this sample selection is likely to bias the results, as it is omitting people with certain behaviors (perhaps, they are healthier). There are methods such as two-part models that can handle the issue of having zeros for some of the processes (e.g., alcohol) and accommodate the entire sample.

   We considered using a two-part model to accommodate the issue of zeroes for alcohol consumption. However, one issue is that zero values for alcohol intake cannot be treated as homogeneous – that is, someone not currently drinking alcohol may have formerly been a
drinker, or they may have never consumed alcohol in their life. These categories have distinct risk profiles [1] and as such we made the conservative decision to exclude all those who had not drank in the year before baseline from our analytic sample (whilst keeping those who had not drank in the past week but admitted to drinking on a monthly/special occasion basis in the previous year). We thought that this specification was actually likely to minimise bias. We have, however, estimated a model whereby all types of non-drinker are included in the final sample (N=6,834 compared to 6,330) and came to essentially the same conclusion (the MCS → Δ Alcohol model was of best fit; age and sex adjusted coefficients for alcohol intercept = 16.34, alcohol slope = 23.03, alcohol β = -0.54, MCS intercept = 51.45, MCS slope = 10.40, MCS β=-0.18, MCS γ = -0.32). We hope that our concerns regarding including the “mixed” group of individuals who were not drinking at baseline, combined with the fact that even upon including them in the analytic sample our conclusions do not substantially change, will be suffice in convincing you that accommodating them into the sample using alternative methods is not necessary.

3. I would suggest that the authors have a first table with just model fit across all specifications. In a separate table they can then include the estimates for the best-fitting model only. They could also include a few key estimates for all the models as well, but not so many, and not in so much detail. All the details could be included as supplementary material.

Thank you for your suggestion. We have revised the presentation of our findings accordingly.

4. In all models, the intercept estimate for MCS reflects the observed initial value, approximately, which makes sense. But this is not the case for alcohol (17.15 vs. 14.6). I'm wondering why this discrepancy, which is true across all models. In a growth curve, this could be due to a lack of centering the slope, but the reason in this model is not evident.

This discrepancy is mostly due to the alcohol intercept being estimated conditional upon gender (in Supplementary Table 3A, the coefficient for being a woman is -7.43 on the alcohol intercept compared to -0.80 on the MCS intercept). The observed mean for alcohol at baseline was 17.04 for men which is reflected in the adjusted intercept presented. In models without adjustment for any covariates the alcohol intercept more closely reflects the observed initial value of alcohol intake in the pooled sample (estimated intercept of 14.49 compared to the observed mean of 14.6).

5. Would it make sense to examine the separate effects of each type of alcohol?

While we agree that some readers might find this interesting, unfortunately, we believe that questions pertaining to specific beverage types are outside the scope of the current manuscript. It is common for investigators to examine the impact of different types of alcohol, often concluding that wine consumption is beneficial but there is ample evidence to suggest that such relationships may be spurious on account of the favourable health and lifestyle characteristics typically adopted by or present in wine drinkers compared to those with a preference for spirits and/or beer [2–4]. As such we are reluctant to provide estimates for specific drink types. We are hopeful that these concerns will persuade you that beverage specific models are unlikely to improve our paper.
6. The ellipsoid should contain 95% (or any other %) of all the data, not just data at baseline.

We apologise for this error. The ellipsoid does in fact represent where 95% of all the data lay. We have corrected this description in the main body of the manuscript as well as included a note with the caption for Figure 2 that briefly states this.

We have also made some minor edits throughout the manuscript to improve readability. Edits are marked on the revised version using bold red font. Once more, we would like to thank you for giving us the opportunity to submit a revised manuscript. We look forward to your response.

Yours sincerely,

Steven Bell and Annie Britton

References


