Author's response to reviews

Title: Problem drinking and exceeding guidelines for 'sensible' alcohol consumption in Scottish men: associations with life course socioeconomic disadvantage in a population-based cohort study

Authors:

David Batty (david@sphsu.mrc.ac.uk)
Heather Lewars (heather@sphsu.mrc.ac.uk)
Carol Emslie (ecarol@sphsu.mrc.ac.uk)
Michaela Benzeval (michaela@sphsu.mrc.ac.uk)
Kate Hunt (kate@sphsu.mrc.ac.uk)

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Author's response to reviews: see over
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Professor Andrea Bucceri
Assistant Editor
BMC-series journals

Dear Professor Bucceri,

MS: 1111258199194842. Problem drinking and exceeding guidelines for ‘sensible’ alcohol consumption in Scottish men: associations with life course socioeconomic disadvantage in a population-based cohort study. G. David Batty, Heather Lewars, Carol Emslie, Michaela Benzeval, & Kate Hunt

Thank you for sending us the referees’ comments on our manuscript, for inviting us to revise and re-submit, and for extending the deadline to 1 August 2008.

We note that, in addition to some minor suggestions for improving the manuscript, the reviewers describe our manuscript as “…clear and well written …” (Professor Rudolf Moos), as having “…many good points…”, and being “…of importance in its field.” (both Dr. Elizabeth Breeze).

We provide a point-by-point response to each of the referees’ comments. Revisions to the manuscript are highlighted in red. We have also made various formatting changes at your request.

We think these changes have improved the quality of the article.

We look forward to hearing from you.

Sincerely,

David Batty
pp. co-authors
Response to reviewers' comments

Reviewer's report -- Rudolf Moos

This manuscript uses data from a representative cohort study of 576 men drawn from the west of Scotland. It identifies an association between the men’s reported socioeconomic adversity in early life and adulthood and an increased risk of exceeding “sensible” alcohol consumption guidelines and of experiencing alcohol-related problems. This is a generally clear and well written paper and I have only a few suggested revisions.

Minor Essential Revisions

1. Given that the entire empirical focus of the study is on men, it would seem reasonable to provide information about the completion rate of the initial and second wave of interviews only for men rather than for the entire sample (page 5). It also would be useful to clarify at this point in the methods that lifelong abstainers were excluded.

OUR RESPONSE: Thank you. We have now provided response percents for each of the two waves of data collection. We have stratified this by gender as we feel it is important in the opening section of the Methods to provide an indication of the proportion of both men and women who participated in data collection, so making it explicit that the reason for not examining socioeconomic variations in alcohol intake/problems in women is due to the very low number of ‘cases’ in this age group, and not a low survey response per se.

2. The results for adult income appear to run counter to the general trend that socioeconomic adversity is associated with heavier drinking. This is a potentially interesting finding and might be clarified. Thus, the finding for income does not seem to be “similar” (page 9) to the finding for social class, but rather “in contrast” to it. In addition, it would be useful to explain how adult income was handled in the development of the relative index of inequality (RII).

OUR RESPONSE: We think this referee has misinterpreted the results for income. As we indicated in the original version of the manuscript: “Markers of socioeconomic position were recoded where necessary so that high values reflect disadvantage……. For the purposes of interpretation, the RII should be regarded as the relative risk of exceeding the stated guidelines or problem drinking in the most disadvantaged group relative to the most advantaged.” Thus, a RII above one denotes increased risk of a given alcohol outcome in the disadvantaged. For income, this is the case in relation to each of the three alcohol outcomes – as it is for most of the other socioeconomic indices. The RII for income is, in fact, therefore entirely consistent with our other results.

Discretionary Revisions

1. Perhaps most important, the term “predictive” that is used in the abstract and discussion probably should be removed in favor of a term like “association”. Because participants were remembering childhood adversity and because their heavy drinking and drinking problems may have (and probably did) precede the initial interview in which information was obtained on adult adversity, it cannot be
assumed that the adversity necessarily predated the drinking. Even in childhood, for example, it may be that youngsters who began drinking heavily at an early age “therefore” left school sooner. In addition, drinking problems as identified by the CAGE, are as likely (or perhaps in this age group even more likely) to be past rather than present problems.

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OUR RESPONSE: We agree that using the term “predictive” is probably an overstatement given that we are dealing with an observational study. While we recognised this at the time of writing, we were attempting to avoid repetitive use of the more appropriate terms as correctly identified by the referee – namely, “association” and “relation.” We have now edited the Abstract and Discussion sections to utilise these.

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2. In the introduction (page 4) it would be of interest to describe more precisely how the magnitude and especially the direction of findings vary by country.

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OUR RESPONSE: In general, the socioeconomic position—alcohol gradient varies according to the economic transition of the country from which the study population was drawn. Thus, in affluent countries, there was an inverse gradient (as we see herein), whereas in low and middle income societies the reverse was apparent – that is, social advantage is associated with an unfavourable level of alcohol consumption. While we could of course include these additional comments in the Introduction section, we think that in doing so the direction of it would change, leading to the expectation by the reader that we were either focusing on a sample of people from a low income community, or we were drawing comparison of the socioeconomic position—alcohol associations between cohorts from low and high income. As this is not the case, we think that further elucidation would be unwise.

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3. The results section now describes both statistically significant and trend findings. The N is large enough that perhaps the focus could be entirely on the significant findings.

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OUR RESPONSE: In the interests of brevity, we do understand this referee’s suggestion, however, given that BMC-PH is a web-based journal, we think this is unnecessary. More fundamentally, selectively reporting only significant findings – if we understand this referee’s point correctly – could be regarded, with some justification, as unscientific.

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Recommendation: Accept after minor essential and discretionary revisions.

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

Declaration of Competing Interests: None

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 have no competing interests

Reviewer's report - Elizabeth Breeze

The authors argue that adulthood socioeconomic circumstances are particularly associated with detrimental patterns of alcohol consumption and problem drinking but that there are childhood influences too.

The topic is important if it helps to guide interventions. The authors have the advantage of a data source that has information about the individuals from throughout their life course and detailed measures of alcohol consumption. There are many good points about the manuscript.

All the comments below are discretionary except for clarifying the dichotomies.

Three measures of alcohol behaviour are used: it would be interesting to know to what extent those qualifying on each indicator are the same or different people. Most of the parameters are strikingly similar but it seems that own social class was a stronger predictor, and housing tenure a weaker predictor of problem drinking than of the other indicators of adverse drinking behaviour. (Table 3). I wouldn’t want to read too much into this but wonder if it is something to do with the culture of what is felt to be problematic.

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OUR RESPONSE: This referee raises an important point. While many of the socioeconomic position—alcohol gradients are indeed similar across multiple indicators of deprivation, it was not in fact the same groups of people who were coded as being a ‘case’ when we derived the three alcohol outcomes. Thus, of the 258 men who reported exceeding ‘sensible’ weekly intake of 21 units, 45% (N=117) of these reported consuming 4 units on one or more days per week. For the 85 male ‘problem’ drinkers in this cohort, 52% exceeded the weekly limit, while a predictably higher proportion (78%) surpassed the daily guidelines. We have now added these findings to the Results section.

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My main problem lies with the RII. I know that this is a measure that is accepted and it has the advantage of enabling comparison across different measures. The difficulty I have with it is that it assumes a linear gradient over the socioeconomic scale and compares the hypothetical people at the 0 and 100% points on the ranking – which means that the parameters tend to be more substantial than they are in a logistic regression that compares groups. I am no expert in this but I know that there is an alternative suggested – it seems more to affect the CI than the point estimate but still estimates f(0)/f(1) so does not get round my feeling that one is exaggerating the situation by using mythical ‘people’ at the ends of the scale. [Ref Biostatistics Advance Access published online on September 28, 2005 Biostatistics, doi:10.1093/biostatistics/kxj002 Relative index of inequality: definition, estimation and inference Jamie C. Sergeant and David Firth ] . I do not insist on a revision but would like to know if the linearity was explored.

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OUR RESPONSE: This is an important point. To explore linearity we added a quadratic term to the model for each of the exposures variables in relation to the alcohol outcomes (see table 3 for full list). As it was only possible to test for linearity for those predictor variables with three or more categories, we did not run analyses for the dichotomously coded family structure, employment status, housing tenure and car ownership. The six remaining variables and three alcohol outcomes
therefore resulted in 18 exposure-outcome permutations. Of these, in only one (housing density) did the quadratic term attain statistical significance. As this exceptional result is likely to be a chance finding, we think it is safe to say that the linearity assumption held in these analyses. We have now added this information to the text.

We have also included the caveat, raised by this referee, that the RII is known to elevate effect sizes, especially in variables with only two categories. We chose to use the RII because it permits comparison of effect across socioeconomic variables with different coding structures. As such, our interest did not lie in the absolute size of these effects.

Discussion
Although adult indicators of socioeconomic position attenuate childhood ones, the authors seem to be saying that the childhood ones work through the adult ones, otherwise I presume there would be less emphasis on education. I would agree that this is likely.

OUR RESPONSE: We agree with this point

Is there scope for reverse causation with respect to car ownership = were the rates of consumption so high that people could have been banned or discouraged from driving?

OUR RESPONSE: Reverse causality is a plausible explanation for the car ownership—alcohol intake association, as it is for selected others in these analyses. For instance, high alcohol intake and its attendant problems could lead to unemployment, reduced income, and loss of car ownership. This issue could be addressed by using alcohol outcomes based on incidence rather than prevalence, but we do not know when the study members became ‘cases’. The problem of reverse causality is unlikely to be germane to the relation between alcohol consumption/problems and early life socioeconomic position when intake is likely to have been non-existent for most, if not all, study members. We have now added this point to the Discussion.

Policy implications
Education is presumably more modifiable for future generations than for existing adults so this is a long-term proposition. Another point that in theory is amenable to intervention is employment status – was the employment history of many of these people unstable; might more stable employment have made the drink less attractive?

OUR RESPONSE: We agree that efforts to increase educational achievement are likely to be most profitable in younger people, and this point has now been added to the manuscript. On the second observation, unfortunately, we do not have lifetime employment histories for these study participants.

Clarification: where the cut-points were made to create dichotomies for the explanatory variables (page 8)

OUR RESPONSE: Thank you. Explanatory variables were dichotomised at the following demarcation points. Father’s and own social class (1=partly skilled manual, unskilled manual);
Family structure (1= circumstance other than having two biological parents in the family at age 15 years); Education (1=left school age 12-14 years); Employment status (1=all other categories but employed [i.e., retired, disabled/ill, caring for the home, in education, unemployed]); Income (1=lowest quartile); Housing tenure (1=all other categories but privately own house [council, rent, job-related, other]); Housing density (1=overcrowded quartile); Car ownership (1=no); Marital status (1=single). This information has now been added to the text.

Discussion p10 last para. Minor point on terminology. Material indicators that were more predictive of heavy drinking – housing tenure was, housing density was not. Suggest replace “housing deprivation” in brackets by “housing tenure”

OUR RESPONSE: Thank you. These revisions have been made to the revised manuscript. We have also replaced “housing density” with “household crowding” as the former implies an area-based measure.

Level of interest: An article of importance in its field

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 have no competing interests