Author's response to reviews

Title: Socioeconomic deprivation, urban-rural location and alcohol-related mortality in England and Wales

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Author's response to reviews:

Melissa Norton, MD
Editor-in-Chief
BMC Public Health

Dear Professor Norton,

Re: Socioeconomic deprivation, urban-rural location and alcohol-related mortality in England and Wales

Revisions to the manuscript in response to reviewers’ comments are detailed below:

Reviewer: Susan Wighton
Reviewer's report:

General comment
This is a very well written and engaging paper and the findings are considerable, with the intelligent disaggregation of available and pertinent statistical data that informs current understanding and policy around alcohol usage and sequelae in contemporary society.

• Thank you.

I would suggest only minor changes that the authors may choose to disregard as they come from another discipline and may not fit with their preferred academic style.

In the abstract Conclusions [p3] regarding the future design of public health policy on alcohol related harms. I think this could be more forcefully stated as the research findings of this paper are significant and the authors deserve credit for them.
• We did consider this but given the second reviewer’s comments regarding the Conclusion and policy statements we have opted to leave the conclusion in the abstract unchanged.

On P4 [Para 1] in the Introduction the percentage of total adult population in England – 8.2 million, may be more expressive of the enormity of the problem.
• We have given the percentages for men and women.

Methods P5 [Para 2] some formats use dual capitalisation on Poisson Regression.
• We generally use single capitalisation and have opted to leave this unchanged.

In P5 [Para 3] it may be useful to name the ONS dataset referred to in relation to alcohol –related mortality.
• The dataset does not have a particular name and we have given the reference to the ONS publication referring to the dataset.

P5 [Para 3] reference to The Carstairs Index may require acknowledgement of existence of SIMD [Scottish Index of Multiple Deprivation] and devolved comparators e.g. WIMD etc.
• We agree there are other very useful indicators. We have opted to keep the focus on Carstairs Index in order not to inadvertently detract from the analysis we undertook.

In Discussion pp 8-9 it may be useful to identify the origins of these comparative papers as global rather than descriptive of UK studies e.g. USA, Finland and Russia.
• We have added a sentence to highlight the above.

In P9 [Para 2] the discussion could perhaps expand further on the authors’ views of what may be occurring in relation to socio economic status and reported alcohol consumption patterns.
• We have altered the start of the paragraph but maintained the rest of the paragraph unchanged as it already contains a number of points.

Reviewer: Johan Jarl

Reviewer’s report:
The paper studies inequalities in alcohol-related deaths based on area deprivation. This is a fine descriptive study. However, it does not account for causality nor does it control for endogeneity, rendering the results insecure and inappropriate for policy discussion. I believe there is room for improvement, some of which I will discuss below. The study can however stand as it is, given that the reader is aware of the descriptive nature of the study.

Minor Essential Revisions
Abstract

Method: It says 9797 wards in the abstract but in the paper the number is 8797. Which is correct?

• 9797 was a typographical error and we have corrected this to 8797.

Methods

Regarding using non-alcoholic liver cirrhosis in the definition of alcohol-related deaths. This approach has been used in prior research and is considered acceptable. However, I have some concerns, especially of using this approach in connection to deprivation. Given that 85% of all deaths included in the article are due to liver cirrhosis, any possible bias will have a large impact on the results. For examples, in a study separating alcohol-attributable liver cirrhosis cases from non-alcohol related cases, the former has been shown to be less than half of total cases (Jarl et al. 2008). In that study was, for mortality, 40% (men) and 32% (women) of all liver cirrhosis deaths considered to be due to alcohol consumption. The corresponding figure for inpatient care was 43 and 36%. If we take these figures as given, for the sake of argument, this would imply that in the current study, 9,545 cases included for men and 5,851 cases included for women are not due to alcohol consumption. That is, about half of the included cases are not alcohol-related. This is only a problem if other risk factors are unevenly distributed based on deprivation. The authors mention diet (fat) as a contributing cause of cirrhosis, but also other causes should be considered (e.g. hepatitis, drug use), which might be expected to have a skewed distribution in society. The results of this study are at risk of picking up this potential skewed distribution of other risk factors as well, probably overestimating the impact of deprivation. I would suggest that the authors do some form of sensitivity analysis, for example excluding liver cirrhosis, and also discuss the definition of alcohol-related deaths as a potential limitation/bias.

• We have added this point to the discussion paragraphs on limitations. We were unable to run the sensitivity analysis as the dataset does not contain deaths in a disaggregated form.

Regarding the ecological approach. How large are the wards used to measure deprivations? Is there a risk that individuals spend a large portion of their waking hours in another ward than in the one in which they reside (e.g. work etc.)? Is this any cause for concern?

• The wards contain approximately 6000 people on average. The concern here is ecological bias and we have acknowledged this in the discussion paragraphs on limitations of the study.

More information about the data set should be included, e.g. coverage, potential limitations, etc.

• We have added a sentence mentioning that the dataset included all standard table wards in England and Wales. The limitations of the dataset are addressed in detail in the Discussion in the paragraphs on limitations.
The paragraph regarding the Poisson regression should be extended. I do not believe that this current estimation method is common knowledge for the readers of BMC Public Health.

• We have expanded the paragraph regarding Poisson regression.

Discussion

An issue that should be discussed is potential endogeneity between alcohol consumption and (living in) deprived areas. What the authors are interested in is if living in deprived areas increase the risk of alcohol-related deaths (high alcohol consumption). But it is also possible that individuals with hazardous consumption tend to move to deprived areas (lower cost for housing, easier to get a apartment contract, etc.). The authors, correctly, only discus associations, but discussing the potential endogeneity is important.

• This key point has been emphasised at the start of paragraph 7 in the Discussion.

Regarding paragraph 4 in the discussion section; the comparison between the results of the study with the General Household Survey. I am not convinced that the Carstairs Index/deprivation and professional group measure the same thing. The question that needs to be dealt with is to what extent do lower socioeconomic group correspond to deprivation.

• We have acknowledged this point in paragraph 4 as a possible explanation for the apparent discrepancy.

Paragraph 5. As I understand it, you do not have any information on drinking behaviour. Thus, you really cannot say if socioeconomically deprived are more likely to suffer alcohol-related death because they drink more or because they are deprived (i.e. other factors as discussed in the paper). That is, the first sentence in paragraph 5 should be rephrased.

• We have rephrased the first sentence.

Conclusions

Due to the descriptive nature of the current study, recommendations for policy decisions should be avoided.

• We have removed the reference and its accompanying policy decisions from the Conclusion.

Tables 1-3

There is too much information in the table headings. These should be shortened and additional information can be included as table footnotes.

• We have moved part of the heading text to a footnote for Table 2. We felt that the information contained in headings for Tables 1 and 3 was useful as a lead in to information provided in the tables.

Discretionary Revisions
Discussion: It is not very surprising that the inequality in risk decreases with age. The individuals most at risk have already attracted the disease at a younger age; income tends to become more equally distributed after retirement; and also health tends to become more equal with higher age. The discussion would benefit from discussion these issues.

- This is an important general point but we were reluctant to expand the Discussion further.

Discussion, paragraph 6. Your discussion here could be broadened to also include inequalities in health care, where deprived areas might get less (more?) resources based on need, resulting in queues or not getting treatment at all. In addition, worth considering is if individuals in deprived areas are less likely to seek medical care (at an early stage), increasing the risk of mortality.

- The paragraph develops a particular point regarding incidence vs survival and we have opted keep the focus as it is.

Figure 1: It is difficult to tell the difference between the curves. May I suggest using signs instead of colours to differentiate them?

- We have amended the figure.

Thank you.

Yours sincerely,

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