Reviewer's report

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

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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?

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.
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?

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

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.

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.

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.

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.

Conclusions

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

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.

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.

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.

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

References

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.