PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Healthier central England or North-South divide? Analysis of national survey data on smoking and high-risk drinking
AUTHORS	Beard, Emma; Brown, Jamie; West, Robert; Angus, Colin; Kaner, Eileen; Michie, Susan

VERSION 1 - REVIEW

REVIEWER	Jon Heron University of Bristol UK
REVIEW RETURNED	02-Nov-2016

GENERAL COMMENTS	I have a few fairly minor comments [1] There is a typo in table 1 – I think the proportion of females among the non-smoking group is probably 47.3% [2] Table 1 seems unnecessarily dense. Would it suffice to include percentages with SE's rather than CI's? [3] Can you make it clear in table 1 that the 1st quantile (actually quartile?) is the highest SES group? [4] I found the use of the phrase "adjusted for" a little strange given the context. We use the term when describing confounders with the notion that we are ideally edging towards a causal estimate and that the adjusted estimates are in some way superior to the raw unadjusted associations. In the current situation this is of course nonsensical. It should be possible to totally explain regional variations in drinking and smoking, were such data available to us.

REVIEWER	Professor Liz Twigg Department of Geography University of Portsmouth England
REVIEW RETURNED	07-Nov-2016

GENERAL COMMENTS	General comments This is a regional analysis of adult smoking prevalence and high risk alcohol use across England and outlines the regional pattern before and after adjustment using a number of socio-economic/demographic variables. The sample size is relatively large (over 40k) and appropriate modelling techniques have been used to undertake the relatively simple analyses.

	I think my main concern is the relative simplicity of the analyses and the usefulness of undertaking a regional analysis. As the paper points out, most stop smoking services and alcohol support services are managed and delivered sub-regionally. It is very important to understand local authority variations as well as sub LA differences (ie across wards or neighbourhoods). I expect that there are small areas of central England that have prevalence rates just as high as those reported for the northern regions. Is the variation of these two risk behaviours within regions greater than between? Are sub-regional data not available? If not then I think the authors need to justify the regional focus in more depth. I would also like to see the model consider interactions between individual characteristics and regional characteristics – for example the relative risks are reported for gender, age, ethnicity and SES but it is very likely that these differ according to region and the main effects of region may be different when interactions are introduced to the model. This should be relatively easy to do and may produce different results. For example the female gender effect is given as a RR of 0.85 for smoking but this is assumed to be universal across all regions. It may be that there is a regional geography to the gender effect. Testing for these interactions would be an interesting exercise and one which may produce more useful intelligence for regional health promotion services. Specific comments Page 2: Limitations -- point 2 affected rather than effected? Page 3: Introduction and elsewhere in manuscript – the numbered citations do not appear to align with the bibliography list at the end. For example citation number 4 is focused on alcohol but is used to cite smoking evidence. I checked the reference and could not find the evidence as suggested in the text. There are other instances in the manuscript where this type of thing happens and the authors need to check that the bibliography is aligned correctly Page 3: Some of the material towards the end of page 3 seems a little superficial. Whilst I appreciate that it is important to get key messages across I think there should be a couple of sentences about the interaction of demographic effects. For example smoking across ethnic groups is not always as described once adjustment is made for age. Likewise social gradients vary according to age. Page 4 – the reference to ward level differences in smoking is quite dated – which is fine but this could be used to argue that a more up to date picture is needed (unfortunately the current paper has not investigated the same level of granularity) but the comparison with the regional findings may be useful all the same. Middle of page 4: What useful strategies could be applied at a regional level – if the findings are policy relevant – the paper should make stronger links to policy frameworks. Table 1: I am not sure how useful this table is at is stands – I think I would rather see row percentages calculated (but if column then the argument needs to be made why this is so). Also I think ‘n’ might be more useful than the CIs? Page 7: Why were the census ethnicity groupings used? Was there

	potential to look at ethnicity in more detail? Page 8 – Analysis – say a little more about weights – ie weighted according to? Page 8 – instead of 'non-ordinal nature' – use 'nominal'? Table 3 – It would be good to test for interactions –see general comments Page 13 – how useful are the concluding remarks/findings? This links to earlier comments about variation within regions. What are the real implications for policy as current services are arranged. I think strengthening the rationale and conclusions would help the paper enormously. Page 15 onwards – bibliography – check the alignment with the numbers in the text Page 24/25 I think the maps could be made more informative – what determines the colour grading - is this the value of the RR or the significance. For the non-uk reader the regions ought to be labelled.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

[1] There is a typo in table 1 – I think the proportion of females among the non-smoking group is probably 47.3%

Thank you for spotting this mistake. We have now updated the analyses so that we had enough power to look at moderation effects (a suggested reviewer amendment). This percentage is now 47.5%.

[2] Table 1 seems unnecessarily dense. Would it suffice to include percentages with SE's rather than CI's?

Although we agree that presenting SE's would be helpful in terms of making the table less dense (and that CI's could be calculated from them), we believe that confidence intervals are useful for the reader. They show the possible values consistent with the population estimate and if there is a significant difference between groups (i.e. if they do not overlap) at the alpha=0.05 level.

[3] Can you make it clear in table 1 that the 1st quantile (actually quartile?) is the highest SES group?

We have now changed quantile to quartile throughout the paper and have added a note to the table that quartile 1 is the high SES group.

[4] I found the use of the phrase "adjusted for" a little strange given the context. We use the term when describing confounders with the notion that we are ideally edging towards a causal estimate and that the adjusted estimates are in some way superior to the raw unadjusted associations. In the current situation this is of course nonsensical. It should be possible to totally explain regional variations in drinking and smoking, were such data available to us.

The goal of this paper was to see if "adjustment" for, or in other words controlling for, socio-demographic characteristics explains to some extent regional variations. We are unaware of another term which could be used, but would be happy to change this if there is a suggestion.

Reviewer: 2

[1] I think my main concern is the relative simplicity of the analyses and the usefulness of undertaking

a regional analysis. As the paper points out, most stop smoking services and alcohol support services are managed and delivered sub-regionally. It is very important to understand local authority variations as well as sub LA differences (ie across wards or neighbourhoods). I expect that there are small areas of central England that have prevalence rates just as high as those reported for the northern regions. Is the variation of these two risk behaviours within regions greater than between? Are sub-regional data not available? If not then I think the authors need to justify the regional focus in more depth.
The STS and ATS studies use a type of random location sampling. This means that each month households within each government office region are sampled but not necessarily within each local authority. Thus, the sample is too small to look at a more micro-level. One analysis we have considered for the future would be to match areas on measures of deprivation (or something similar). Researchers at Bristol University are currently using other data sets in a similar manner.
One of the aims of this study was to provide an update, and extension of, the findings from other surveys which have also stratified smoking and high-risk drinking by government office region. To date this has been the standard way of splitting England as regions reflect strategic health authorities in place up until 2013.
We had discussed this briefly in the limitations but have now added “This paper also only considered patterns of smoking and alcohol use at the Government Office Region level, an approach taken by other population surveys (e.g. the Health Survey for England and Integrated Household Survey) due to the historical link until April 2013 with strategic health authorities. This ensured enough power to be able to assess the impact of socio-demographic characteristics on regional variations. Since then commissioning of stop smoking and alcohol services has been moved to local authority control; thus, future studies may wish to consider variation at this more micro-geographical level [83].

[3] I would also like to see the model consider interactions between individual characteristics and regional characteristics – for example the relative risks are reported for gender, age, ethnicity and SES but it is very likely that these differ according to region and the main effects of region may be different when interactions are introduced to the model. This should be relatively easy to do and may produce different results. For example the female gender effect is given as a RR of 0.85 for smoking but this is assumed to be universal across all regions. It may be that there is a regional geography to the gender effect. Testing for these interactions would be an interesting exercise and one which may produce more useful intelligence for regional health promotion services.
We agree that moderation effects would be an interesting addition to the analysis plan. Interaction effects are now reported in the results and covered in the discussion.

[4] Page 2: Limitations -- point 2 affected rather than effected?
Thank you for spotting this grammatical error

[5] Page 3: Introduction and elsewhere in manuscript – the numbered citations do not appear to align with the bibliography list at the end. For example citation number 4 is focused on alcohol but is used to cite smoking evidence. I checked the reference and could not find the evidence as suggested in the text. There are other instances in the manuscript where this type of thing happens and the authors need to check that the bibliography is aligned correctly
All references and alignments have been checked.

[6] Page 3: Some of the material towards the end of page 3 seems a little superficial. Whilst I appreciate that it is important to get key messages across I think there should be a couple of sentences about the interaction of demographic effects. For example smoking across ethnic groups is not always as described once adjustment is made for age. Likewise social gradients vary according to age.
As we have now included interaction effects we have added the following sentences to the introduction: “Many of these previous studies also did not adjust for the full range of possible socio-

demographic variables, including ethnicity [38] and/or did not consider possible moderation effects, for example, social-gradients in alcohol consumption tend to differ by age and gender [41]."
We also added the following to the aims: "A secondary aim was to assess whether regional patterns were similar across socio-demographic subgroups by looking at moderation effects."

[7] Page 4 – the reference to ward level differences in smoking is quite dated – which is fine but this could be used to argue that a more up to date picture is needed (unfortunately the current paper has not investigated the same level of granularity) but the comparison with the regional findings may be useful all the same.
We agree that an update is needed at both the regional and more micro-geographical level. This study is reported as we could find only a handful of studies which considered regional variations as a function of socio-demographic characteristics.

[8] Middle of page 4: What useful strategies could be applied at a regional level – if the findings are policy relevant – the paper should make stronger links to policy frameworks.
We have addressed this in the discussion as follows: Policies of allocating a greater proportion of health resources to poorer areas has been shown to be associated with declining inequalities in mortality amenable to healthcare [79], while decreases in unemployment in deprived areas prior to the economic crisis were associated with reductions in inequalities in male life expectancy between areas [80]. Local areas in England with more intense alcohol licensing policies have also seen stronger declines in rates of violent crimes, sexual crimes and public order offences. The capacity of the UK Government to address regional imbalances has been limited somewhat by the dismantling of regional administrative structures in recent years, including Government Offices, Regional Development Agencies and Strategic Health Authorities [60]. This has resulted in a fragmented system of support for smokers and dependent drinkers. Although stop smoking services are one of the most cost-effective life preserving services offered by local authorities and local level intensive alcohol licensing policies (known as Cumulative Impact Zones) have been shown to result in declines in rates of violent crimes, sexual crimes and public order offences; substantial variation exists across England in terms of their effectiveness and scale of implementation [55 66-68]."

[9] Table 1: I am not sure how useful this table is at is stands – I think I would rather see row percentages calculated (but if column then the argument needs to be made why this is so). Also I think 'n' might be more useful than the CIs?
Although we agree that presenting n's would be helpful we believe that confidence intervals are more useful for the reader. They show the possible values consistent with the population estimate and if there is a significant difference between groups (i.e. if they do not overlap) at the alpha=0.05 level.

[10] Page 7: Why were the census ethnicity groupings used? Was there potential to look at ethnicity in more detail?
Previous studies have categorised individuals according to these or the simple dichotomy white versus ethnic minority. Although the STS and ATS record ethnicity in more detail (e.g. white British, white Irish, white gypsy/traveller, white other, mixed white/black Caribbean, mixed white/black African, mixed white/Asian, mixed other etc) the sample sizes in most of these categorises would have been too small for the planned analyses.

[11] Page 8 – Analysis – say a little more about weights – ie weighted according to?
We have added the following information on weighting: "Descriptive statistics were weighted for the STS and ATS using a rim (marginal) weighting technique. This involves an iterative sequence of weighting adjustments whereby separate nationally representative target profiles are set (for gender, working status, children in the household, age, social-grade and region). This process is then repeated until all variables match the specified targets"

[12] Page 8 – instead of ‘non-ordinal nature’ – use ‘nominal’?
We have made this change

[13] Table 3 – It would be good to test for interactions –see general comments
Interactions have been run and are included as sensitivity analyses and results are shown graphically

[14] Page 13 – how useful are the concluding remarks/findings? This links to earlier comments about variation within regions. What are the real implications for policy as current services are arranged. I think strengthening the rationale and conclusions would help the paper enormously.
We have now included a decision of the limitation of being unable to look at more micro levels in the current paper (please see response to previous comments).

[15] Page 15 onwards – bibliography – check the alignment with the numbers in the text
These have now been checked.

[16] Page 24/25 I think the maps could be made more informative – what determines the colour grading - is this the value of the RR or the significance. For the non-uk reader the regions ought to be labelled
The graphs show the relative risk difference using colours (darker red or blue = larger risk) for regions relative to the south west. Regions with a lower risk are coloured blue and with a higher risk are coloured red. Regions which did not have a statistically significantly different risk are coloured white. We hope this is clear from the legend but have amended the figure descriptions to make this clearer. We have also added a graph as supplementary material which labels the regions.

VERSION 2 – REVIEW

REVIEWER	Professor Liz Twigg Department of Geography University of Portsmouth England
REVIEW RETURNED	03-Jan-2017

GENERAL COMMENTS	This is much improved manuscript. I just have one final query - I don't quite understand how the error bars in the Relative Risk graphs in the Supplementary files (moderation effects) can straddle 0 when they are designated as significant. Is this because of a non-strict use of the 0.05 cut-off etc or am I misinterpreting things. Perhaps a line of explanation on the diagrams may be needed?

VERSION 2 – AUTHOR RESPONSE

Review 1:
1. This is much improved manuscript. I just have one final query - I don't quite understand how the error bars in the Relative Risk graphs in the Supplementary files (moderation effects) can straddle 0 when they are designated as significant. Is this because of a non-strict use of the 0.05 cut-off etc or am I misinterpreting things. Perhaps a line of explanation on the diagrams may be needed?

These graphs show the results of the interaction (moderation analysis). If the CI does not straddle 0 then there is a significant difference between that region and the South West (reference region) in the risk of smoking or high-risk drinking. A significant interaction effect is evident if CI’s for the different subgroups do not overlap with each other i.e. the relative risk difference is different for subgroup a and subgroup b (for example, men from London and Women from London). We have added the following note under the graphs to explain this: “Note: Bars reflect 95% confidence intervals;

Confidence intervals which do not straddle 0 indicate a significantly different risk of high-risk drinking in that region relative to the South West for the subgroup under investigation; ‘a’ and ‘b’ reflect a significant interaction effect at $p<0.05$ i.e. the relative risk difference of high-risk drinking for sub-group ‘a’ is significantly different to subgroup ‘b’”