PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Socioeconomic patterning of excess alcohol consumption and binge drinking: a cross-sectional study of multilevel associations with neighbourhood deprivation
AUTHORS	Fone, David; Farewell, Daniel; White, James; Lyons, Ronan; Dunstan, Frank

VERSION 1 - REVIEW

REVIEWER	Kim Bloomfield professor Centre for Alcohol and Drug Research Aarhus University Denmark I have no competing interests regarding the review of this manuscript.
REVIEW RETURNED	12-Dec-2012

GENERAL COMMENTS	This paper represents a solid piece of research into the relationship of area-level deprivation to alcohol use based on a large representative sample of the Welsh general population. It offers new information on how different drinking behaviours are differentially related to neighbourhood SES. A general comment The authors should avoid using abbreviations of classification systems as a substitute for a variable name. Abbreviations which do not refer to an actual description of the variable are confusing and annoying for the reader. For example, instead of “NS-SEC3” the authors could write “occupational status” – or an abbreviation of that. Specific comments Introduction The introduction is well written and concise. The only comment here is to explain what is meant by “joint effects” used in line 32 of page 8. Do the authors simply mean “simultaneous effects”? Or does “joint” men something different? Methods Please move the information on response rates, which are given in the discussion section, to the participant section. The authors should also provide in the paper a brief description of the sampling approach. There should be some additional information on how the drinking variable was asked: was it only to find out the highest number of drinks consumed on any one day in the last 7 days, or were participants asked about their drinking on every day of the last 7

days (i.e., Weekly recall approach)? If it was the latter, then the authors should explain and justify why they chose to focus only on the maximum number of drinks on any one day in the last 7 and there should be a discussion of the biases in drinking measurement that this approach will have (in the discussion).

Why did the authors choose to create ordinal categories instead of taking the number of drinks as a more “true” and original variable the actual number of drinks?

Please provide more information on the WIMD2005. How was the score created? What is it comprised of? Are the elements of the score weighted? What is the range of values of the score? How does it compare with other commonly used measures in the field, e.g., Townsend, Carstairs?

Please give more description of the LSOAs, for example, describe the range of kinds of neighbourhoods: inner city ghettos composed of whom? Wealthy suburban areas? It would help if the reader is offered a sense of what the neighbourhoods are like.

Who are the “data owners”? p. 10, line 16.

Why have the authors also chosen to look at the unitary authorities? What additional information would they bring?

The sub-title “Measure of SES and potential confounding variables” should be changed to read “Measure of individual SES and potential confounding variables”. In a paper addressing multi-level effects it is always important to clearly differentiate the various variables on the various levels. Otherwise the reader is very quickly confused.

The first sentences in this section are confusing. The authors state that they are using a measure of income as the principal measure of SES: the NS-SEC3 but this does not seem to be a measure of income but rather of occupation. Furthermore, is there a danger of over-controlling with the inclusion of employment status and education? How did the authors decide to use the NS-SEC3 and not education, for example? This section should be re-written and the decision strategy made clearer.

Results
Multilevel models
As someone not familiar with continuation ratio models, it is difficult to interpret the parameter estimates in models 1 and 2 (table 3) which all have increasingly negative values with regard to both outcome measures. One can easily see the relationship to excess consumption, but not to the binge drinking outcome in which the percentages actually increase. It would be good if the authors could better explain this result and how the two outcome points “hang together” with the parameter estimates.

The paragraph regarding random effects variance (table 4) is awkwardly written. For example, it is not clear to what variable the “inter-quartile” range refers. Further on what basis can the authors say that the size of the variation suggests “important” unexplained variance (line 47, page 17). On what basis can importance be judged? It would benefit from a re-write and also inclusion of a rationale for why these analyses were conducted. Additionally table 4 could be better labelled and inclusion of the standard error might

	be more informative than the standard deviation. Discussion The authors could elaborate on what they mean by a "substantial geographical effect of neighbourhood…", line 32, p. 19. In what way was the effect geographical? Until now there has been no discussion of geography. On line 32, page 20 the authors state that "the deprivation-density hypothesis could not explain the findings..-", but this was not tested in their analyses, and could also be mentioned as a limitation of the present research.

REVIEWER	Katherine J. Karriker-Jaffe Associate Scientist Alcohol Research Group Public Health Institute Emeryville, CA USA I have no competing interests to declare.
REVIEW RETURNED	04-Jan-2013

THE STUDY	The abstract would be improved by inclusion of additional details for an international audience (for example, statement of the UK alcohol consumption guidelines and clarification of the levels of the categorical outcome). Likewise, in the measurement section, more information about the indicator of neighborhood deprivation would be helpful for readers not familiar with the Welsh Index of Multiple Deprivation (specifically, list the indicators included in the index and provide some evidence of its reliability and validity). Otherwise, the study is clearly described and the STROBE statement seems adequate.
RESULTS & CONCLUSIONS	The presentation of the results would be enhanced by (1) provision of bivariate statistics to support the descriptive analyses, (2) presentation of parameter estimates for the significant interaction terms from the multivariate models as well as estimates of the simple slopes to accompany the figures [to indicate the groups for which neighborhood deprivation is significantly associated with the different levels of drinking], (3) inclusion of additional information about the interpretation of the coefficients from the multilevel continuation ratio models, and (4) conversion of the random effects variance estimates into intraclass correlation coefficients for ease of interpretation and comparison between levels and across models. The association between neighborhood disadvantage and the heavy drinking outcome may be stronger were the model(s) limited to drinkers. As the analyses stand presently, all non-drinkers are included in the reference group with drinkers who do not consume in excess of the recommended guidelines. It would be interesting to see whether/how things changed if non-drinkers were excluded from the analyses. This is particularly important if, in the UK, lower SES (both individual and neighborhood) is positively associated with both abstinence and heavy drinking by drinkers (as some evidence suggests is the case in the USA). The contrast between the neighborhood- and individual-level correlates of excess consumption and binge drinking is very interesting. In the discussion of the findings, the authors present

	three possible mechanisms by which neighborhood deprivation might be related to both excess consumption and binge drinking. The paper would be much stronger if there were some analyses addressing these hypotheses (even if they were preliminary, post hoc explorations of the data). If there are no such data available in the survey, the authors should state this as a clear limitation of the study.
GENERAL COMMENTS	The figures are interesting but difficult to understand when presented in black and white (grayscale). The formats of the lines should be changed so they are more visually differentiated.

VERSION 1 – AUTHOR RESPONSE

Reviewer: Kim Bloomfield
professor
Centre for Alcohol and Drug Research
Aarhus University
Denmark

I have no competing interests regarding the review of this manuscript.

This paper represents a solid piece of research into the relationship of area-level deprivation to alcohol use based on a large representative sample of the Welsh general population. It offers new information on how different drinking behaviours are differentially related to neighbourhood SES.

A general comment
The authors should avoid using abbreviations of classification systems as a substitute for a variable name. Abbreviations which do not refer to an actual description of the variable are confusing and annoying for the reader. For example, instead of “NS-SEC3” the authors could write “occupational status” – or an abbreviation of that.

We have changed NS-SEC3 to ‘occupational social class’ at the first mention and ‘social class’ thereafter.

Specific comments
Introduction
The introduction is well written and concise. The only comment here is to explain what is meant by “joint effects” used in line 32 of page 8. Do the authors simply mean “simultaneous effects”? Or does “joint” men something different?

By joint effects we mean that we are investigating the simultaneous main effects and the multiplicative interaction in the same model. For simplicity and to avoid misunderstanding we have deleted the word ‘joint’ from both the introduction and the abstract.

Methods
Please move the information on response rates, which are given in the discussion section, to the participant section. The authors should also provide in the paper a brief description of the sampling approach.

We have done this, on page 9. As a consequence of moving the response rate text, the subsequent reference numbers have changed.

There should be some additional information on how the drinking variable was asked: was it only to find out the highest number of drinks consumed on any one day in the last 7 days, or were participants asked about their drinking on every day of the last 7 days (i.e., Weekly recall approach)? If it was the latter, then the authors should explain and justify why they chose to focus only on the maximum number of drinks on any one day in the last 7 and there should be a discussion of the biases in drinking measurement that this approach will have (in the discussion).

The survey question was indeed ‘the highest number of drinks consumed on any one day in the last 7 days’ – this is stated in the first sentence of the ‘Alcohol outcome measure’ section on page 9 and clarified in table 1.

Why did the authors choose to create ordinal categories instead of taking the number of drinks as a more “true” and original variable the actual number of drinks?

The focus of our study was to investigate the UK policy problem of binge drinking and differences from excess consumption. The variable was created by the data owners – the Welsh Government – based on the UK Department of Health definition for excess consumption and binge drinking. An analysis of the number of units consumed would be the focus of a separate paper.

Please provide more information on the WIMD2005. How was the score created? What is it comprised of? Are the elements of the score weighted? What is the range of values of the score? How does it compare with other commonly used measures in the field, e.g., Townsend, Carstairs?

We have added all this requested extra information on the WIMD2005 on page 10, and added an additional reference for the Townsend index..

Please give more description of the LSOAs, for example, describe the range of kinds of neighbourhoods: inner city ghettos composed of whom? Wealthy suburban areas? It would help if the reader is offered a sense of what the neighbourhoods are like.

We have added extra information on the LSOA neighbourhoods on page 10/11.

Who are the “data owners”? p. 10, line 16.

We have clarified that the data owners are the Welsh Government, now on page 11.

Why have the authors also chosen to look at the unitary authorities? What additional information would they bring?

The unitary authority is an important level in the multilevel analysis as this area of local government is responsible for alcohol licensing policy. Thus it is of importance to assess the random effects variance at this level since differences in licensing policy between local authorities could be associated with varying levels of consumption.

The sub-title “Measure of SES and potential confounding variables” should be changed to read “Measure of individual SES and potential confounding variables”. In a paper addressing multi-level effects it is always important to clearly differentiate the various variables on the various levels. Otherwise the reader is very quickly confused.

We have made this change.

The first sentences in this section are confusing. The authors state that they are using a measure of income as the principal measure of SES: the NS-SEC3 but this does not seem to be a measure of income but rather of occupation.

We have clarified the NS-SEC3 query on page 11 – it is a household-level variable based on the occupation of the occupant with the highest income where the household includes more than 1 working age adult –it is not based per se on income.

Furthermore, is there a danger of over-controlling with the inclusion of employment status and education? How did the authors decide to use the NS-SEC3 and not education, for example? This section should be re-written and the decision strategy made clearer.

We have made the decision strategy clearer on page 11 i.e. we considered other available measures of SES that were associated with alcohol consumption in the dataset. We did in fact include both NS-SEC3 and education in the model since both were associated in bivariate analyses with levels of consumption.

Results

Multilevel models

As someone not familiar with continuation ratio models, it is difficult to interpret the parameter estimates in models 1 and 2 (table 3) which all have increasingly negative values with regard to both outcome measures. One can easily see the relationship to excess consumption, but not to the binge drinking outcome in which the percentages actually increase. It would be good if the authors could better explain this result and how the two outcome points "hang together" with the parameter estimates.

We are grateful for the opportunity to clarify the point, which requires extra information from the model output to be included in our revised table 3. We did not present the estimates from the interactions between the WIMD and the change between categories of consumption in the original submitted table 3 and we apologise for this oversight. The predicted probabilities are derived from both the main effects and interaction estimates (which are additive) and so there is a direct relationship between the magnitude and sign of the combined estimates and the predicted probabilities.

We have clarified and explained this point with the additional text below in the methods (on page 13):

To allow increased flexibility in understanding the effects of deprivation on alcohol consumption, interactions between the change in alcohol consumption category and deprivation quintile were included in the continuation ratio models. The predicted probabilities of excess consumption and binge drinking are derived from the sum of the additive main effect and interaction coefficients.

The paragraph regarding random effects variance (table 4) is awkwardly written. For example, it is not clear to what variable the "inter-quartile" range refers. Further on what basis can the authors say that the size of the variation suggests "important" unexplained variance (line 47, page 17). On what basis can importance be judged? It would benefit from a re-write and also inclusion of a rationale for why these analyses were conducted. Additionally table 4 could be better labelled and inclusion of the standard error might be more informative than the standard deviation.

We have re-written this section for clarity (page 19). The inter-quartile range refers to the distribution of the random effects variance for LSOAs and unitary authorities. The importance is measured by comparison to the magnitude of the parameter estimates for the social class variable. These analyses were conducted because this is one of the advantages of multilevel models – that the random variance is explicitly modelled to allow insight into the contribution of the different geographical levels in the model. We have re-labelled table 4 (and added the intra-class correlation (%) as requested by reviewer 2). The R software we used does not produce the standard error as a measure of the variability of the variance of random effects due to the skewed distribution and the non-negative variance constraint.

Discussion

The authors could elaborate on what they mean by a “substantial geographical effect of neighbourhood…”, line 32, p. 19. In what way was the effect geographical? Until now there has been no discussion of geography.

We considered that the effect was geographical as it referred to variability between neighbourhoods. In response we have amended the text to ‘We found a substantial variation between neighbourhoods, since…’

On line 32, page 20 the authors state that “the deprivation-density hypothesis could not explain the findings..-“, but this was not tested in their analyses, and could also be mentioned as a limitation of the present research.

It was outwith the possibilities given by our dataset to investigate the deprivation-density hypothesis. We suggested that future research should investigate this – an indeed we have been funded to do so (please see reference 40). We have added a sentence to suggest that this is a limitation of the present study, on page 23, as also requested by the other reviewer.

Reviewer: Katherine J. Karriker-Jaffe
Associate Scientist
Alcohol Research Group
Public Health Institute
Emeryville, CA USA

I have no competing interests to declare.

The abstract would be improved by inclusion of additional details for an international audience (for example, statement of the UK alcohol consumption guidelines and clarification of the levels of the categorical outcome). Likewise, in the measurement section, more information about the indicator of neighborhood deprivation would be helpful for readers not familiar with the Welsh Index of Multiple Deprivation (specifically, list the indicators included in the index and provide some evidence of its reliability and validity).

We have implemented these suggestions.

Otherwise, the study is clearly described and the STROBE statement seems adequate.

Thankyou

The presentation of the results would be enhanced by (1) provision of bivariate statistics to support the descriptive analyses

Please forgive us if we have misunderstood this suggestion, but table 1 contains bivariate statistics for excess alcohol consumption and binge drinking for each category of the exposure variables.

(2) presentation of parameter estimates for the significant interaction terms from the multivariate models as well as estimates of the simple slopes to accompany the figures [to indicate the groups for which neighborhood deprivation is significantly associated with the different levels of drinking],

We didn't include this information in the submitted manuscript due to the large number of estimates necessary – a total of 60 interaction estimates, plus 12 for the main effects of WIMD, sex and age groups. A table containing this information would be substantially long and so we chose to present this information in the figures. We consider that the figures are much easier to interpret.

(3) inclusion of additional information about the interpretation of the coefficients from the multilevel continuation ratio models,

We have done this as per our response above to reviewer 1.

and (4) conversion of the random effects variance estimates into intraclass correlation coefficients for ease of interpretation and comparison between levels and across models.

We have added this to table 4

The association between neighborhood disadvantage and the heavy drinking outcome may be stronger were the model(s) limited to drinkers. As the analyses stand presently, all non-drinkers are included in the reference group with drinkers who do not consume in excess of the recommended guidelines. It would be interesting to see whether/how things changed if non-drinkers were excluded from the analyses. This is particularly important if, in the UK, lower SES (both individual and neighborhood) is positively associated with both abstinence and heavy drinking by drinkers (as some evidence suggests is the case in the USA).

This is an interesting point. We considered this in detail in planning our study. We decided that in the absence of a variable in the dataset which unequivocally defined a 'never drinker' we would continue to use the given defined outcome variable in which the first category is 'no alcohol in past 7 days/never drinks'. The purpose of our study was to model associations between neighbourhood and individual age/sex and SES and excess consumption and binge drinking in a continuation ratio model, which predicts the probability of moving to the next category conditional on reaching the previous category. Although the probability of drinking within guidelines will not be unbiasedly estimated in our analysis for either never drinkers or subjects who reported not drinking in the past 7 days, it will have no effect on our outcomes of interest i.e. moving from 'within guidelines' to 'excess consumption', nor from 'excess consumption' to 'binge drinking'. The modelling method does not use the first category of 'no alcohol in past 7 days/never drinks' as a reference group and the so the association between neighbourhood disadvantage and the study outcomes will not be different if the model(s) are limited to drinkers.

The contrast between the neighborhood- and individual-level correlates of excess consumption and binge drinking is very interesting. In the discussion of the findings, the authors present three possible mechanisms by which neighborhood deprivation might be related to both excess consumption and binge drinking. The paper would be much stronger if there were some analyses addressing these hypotheses (even if they were preliminary, post hoc explorations of the data). If there are no such data available in the survey, the authors should state this as a clear limitation of the study.

Unfortunately, as the reviewer surmises, data are not available in the survey to address the stated possible mechanisms. We add this as a limitation of the study on page 24.

The figures are interesting but difficult to understand when presented in black and white (grayscale). The formats of the lines should be changed so they are more visually differentiated.

We have amended the figures to label the age group of each line. Thus the lines are now easily visually differentiated in black and white (grayscale), as well as in colour.

VERSION 2 – REVIEW

REVIEWER	Kim Bloomfield, Dr.P.H. Professor Centre for Alcohol and Drug Research Aarhus University, Denmark
REVIEW RETURNED	05-Feb-2013

RESULTS & CONCLUSIONS	The revised manuscript is much improved, but unfortunately not being a statistician, I am still having problems to understand the results in table 3. I had requested clarification from the authors regarding interpretion of the outcome variable in continuation ratio models for the revision but now unfortunately the inclusion of the interaction terms confuse me even more. Now I have to ask how one can create interaction terms between a predictor and an outcome variable? It would be most helpful if the authors could explain this new information in the results section, which they so far have omitted. There is a real need in the paper for the authors to walk the reader through this table, explaining each block of results. Also, I have a question regarding figure 2: Is it true that the probability of bingeing was highest in males 18-34 years or in males 25-34 years (page 21)? And isn't interaction largest in the 18-24 year old group? Or am I reading the graphs incorrectly? There is also no comment made about the ICCs now included in Table 4. They should be explained/described in the results section in the paragraph pertaining to the table.
GENERAL COMMENTS	I find the first sentence in the Discussion section too unwieldly. It would benefit from being cut up into at least two sentences. The three hypotheses cited in the discussion are not referenced. It would be helpful to readers to do so. Finally I suggest only that a concluding sentence or two reminding the reader of what the WIMD2005 is in concrete terms would bring the paper back to the reality of what deprivation means and its relationship to excessive and binge drinking.

REVIEWER	Katherine J. Karriker-Jaffe Associate Scientist Alcohol Research Group Emeryville, CA USA I have no competing interests to report.
REVIEW RETURNED	25-Feb-2013

RESULTS & CONCLUSIONS	I must reiterate my request for bivariate tests of associations to back up the conclusions drawn from and statements made about Table 2 (on p. 14, in particular). Were proportions compared using chi-square tests, for example? If so, values should be reported. The intraclass correlation should not be a percentage. It typically is a number ranging from 0-1 like a correlation coefficient. Please check this with a statistician.
GENERAL COMMENTS	I agree with Reviewer 1 that the use of unnecessary abbreviations (such as NS-SEC3) can be annoying to readers. With this point in mind, I also suggest that the authors replace all references to WIMD2005 with a phrase such as "neighborhood deprivation" for ease of reading.

VERSION 2 – AUTHOR RESPONSE

Reviewer: Kim Bloomfield
professor
Centre for Alcohol and Drug Research
Aarhus University
Denmark

The revised manuscript is much improved, but unfortunately not being a statistician, I am still having problems to understand the results in table 3. I had requested clarification from the authors regarding interpretion of the outcome variable in continuation ratio models for the revision but now unfortunately the inclusion of the interaction terms confuse me even more. Now I have to ask how one can create interaction terms between a predictor and an outcome variable? It would be most helpful if the authors could explain this new information in the results section, which they so far have omitted.

We are very happy to provide further clarification of the continuation ratio model. First, we add this extra paragraph into the methods section on page 12/13:

"To model the variation in the four-category ordinal alcohol consumption outcome using a continuation ratio model, we defined three additional binary explanatory variables, one for each transition between the alcohol outcome categories to indicate the level at which the transition was occurring".
Second, we further clarify the use of interaction terms, which are not between the explanatory variables and the outcome but rather between two explanatory variables i.e. neighbourhood deprivation and the additional explanatory variables for the three binary transitions, by amending the text as follows on page 13:

"To allow a better understanding of the effects of deprivation on alcohol consumption, we fitted interactions between the neighbourhood deprivation quintiles and each additional explanatory variable indicating the relevant binary transition. The predicted probabilities of excess consumption and binge drinking were derived from the sum of these main effects and relevant interaction coefficients."

There is a real need in the paper for the authors to walk the reader through this table, explaining each block of results.

Following on from our response to the above reviewer's comments, we have re-written the paragraphs on page 16 which interpret table 3:

"The model 1 parameter estimates for the neighbourhood deprivation fixed effects and the interaction effects are shown in table 3, together with the unadjusted model predicted probabilities for the five neighbourhood deprivation quintiles. The probabilities of excess consumption and binge drinking were computed from the sum of the fixed and interaction estimates for each neighbourhood deprivation quintile. As we found in the descriptive analysis, the probability of excess consumption was higher in less deprived neighbourhoods with decreasing probability across the quintiles of deprivation. Binge drinking showed the opposite pattern of increasing probability with higher deprivation. The differences in magnitude between the model predicted probabilities and the descriptive data shown in table 2 are explained by the addition of the random effects in model 1.

Table 3 then shows the estimates for the neighbourhood deprivation fixed and interaction effects from model 2, which included social class, age group, gender, the interaction between age group and gender, and the other confounding variables. The sum of the estimates for the fixed and interaction effects for the neighbourhood deprivation quintiles were used as in model 1 to compute the probabilities of excess consumption and binge drinking. In this adjusted model, the difference between the deprivation quintiles for the probability of binge drinking increased, with less effect on the excess consumption category. Respondents in the most deprived neighbourhoods were more likely to binge drink than in the least deprived (adjusted estimates: 17.5% vs. 10.6%; difference in proportions = 6.9%, 95% CI: 6.0 to 7.8), but were less likely to report excess consumption (17.6% vs. 21.3%; difference in proportions = 3.7%, 95% CI: 2.6 to 4.8).

Table 3 finally shows the predicted probabilities of consumption for the SES categories in the fully adjusted model 2. There was little variation in excess consumption with SES. The descriptive analysis finding of a higher probability of binge drinking in the three higher social class groups compared to the never worked/long-term unemployed category remained after adjustment".

Also, I have a question regarding figure 2: Is it true that the probability of bingeing was highest in males 18-34 years or in males 25-34 years (page 21)?

Thanks for spotting this typographical error – it should read '25-34' and we have made this correction.

And isn't interaction largest in the 18-24 year old group? Or am I reading the graphs incorrectly?

Thanks to the reviewer for noting this. We focussed on the increase in binge drinking interpretation in the paper and didn't make the description of the interaction effects in the 18-24 age group sufficiently clear. We have answered this point by amending the text on page 20 as follows:

"The interaction effects suggested that males in the 35-64 year age groups showed the steepest increase in the probability of binge drinking associated with increasing neighbourhood deprivation, while the interaction effect in the 18-24 year age group suggested a weaker association of increasing binge drinking with increasing deprivation."
We have also similarly amended the Results section of the abstract.

There is also no comment made about the ICCs now included in Table 4. They should be explained/described in the results section in the paragraph pertaining to the table.

We have done this as requested, on page 19.

I find the first sentence in the Discussion section too unwieldly. It would benefit from being cut up into at least two sentences.

We have implemented this suggestion.

The three hypotheses cited in the discussion are not referenced. It would be helpful to readers to do so.

We have implemented this suggestion, by adding in a further 4 references.

Finally I suggest only that a concluding sentence or two reminding the reader of what the WIMD2005 is in concrete terms would bring the paper back to the reality of what deprivation means and its relationship to excessive and binge drinking.

We have implemented this suggestion, on page 23.

Reviewer: Katherine J. Karriker-Jaffe
Associate Scientist
Alcohol Research Group
Public Health Institute
Emeryville, CA USA

I have no competing interests to report.

I must reiterate my request for bivariate tests of associations to back up the conclusions drawn from and statements made about Table 2 (on p. 14, in particular). Were proportions compared using chi-square tests, for example? If so, values should be reported.

We compared the proportions with $\chi2$ tests for 2-category variables and $\chi2$ for Trend for 3 or more-category variables. All p-values were $p<0.001$. We have added a note to this effect in table 2.

The intraclass correlation should not be a percentage. It typically is a number ranging from 0-1 like a correlation coefficient. Please check this with a statistician.

Two co-authors are statisticians and we respectfully disagree. Percentages and proportions can be used interchangeably in all contexts, including that of ICCs. We present ICC% in the paper.

I agree with Reviewer 1 that the use of unnecessary abbreviations (such as NS-SEC3) can be annoying to readers. With this point in mind, I also suggest that the authors replace all references to WIMD2005 with a phrase such as "neighborhood deprivation" for ease of reading.

We have implemented this suggestion throughout, including the figures, after the first mention.