PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Do adult adult obesity rates in England vary by insecurity as well as by inequality? An ecological cross-sectional study
AUTHORS	Ulijaszek, Stanley

VERSION 1 - REVIEW

REVIEWER	Noel Cameron Centre for Global Health ad Human Development School of Sport, Exercise and Health Sciences Loughborough University Loughborough UK
REVIEW RETURNED	08-Jan-2014

GENERAL COMMENTS	The idea that inequality and obesity are linked appears to be completely reasonable in a cross-national perspective. Its translation to different areas of a single country in which national development is replaced by a measure of "social class" also appears to be reasonable. The finding that this distribution within a country is refined by considering "insecurity" also appears to be reasonable. My concerns are twofold; firstly, the measure of social class uses a Z-score distribution but neither this author nor the original paper (Savage et al 2013) explains what the Z-score represents i.e. what variable is being standardised. This paper, and indeed the original, would make a lot more sense if it explained what was being standardised to arrive at the social class categories. Secondly, I am concerned that the analysis has been carried without control for gender. Indeed without such control I am unsure of the significance of the findings. It is well known that males and females respond differently to social and economic pressures that drive the prevalence of obesity. The fact that poor, economically and socially insecure adults have the greatest prevalences of obesity is, I would have thought, extremely well known whatever the method used to classify social strata. I am not convinced that the current analysis adds significantly to our understanding of obesity or its prevalence according to social constructs. If there was a "reject and resubmit" category then that is what I would suggest.

REVIEWER	CASTETBON Katia French Institute for Health Surveillance, France
REVIEW RETURNED	24-Jan-2014

GENERAL COMMENTS	This is a highly interesting and original analysis, and results are important to better reference the social variations in obesity. However, the paper would greatly benefit from a revised presentation. Hereby are enclosed comments and suggestions aimed at improving understanding for readers.
	- The title should include the type of study.
	- The section "strengths and limitations" could be more specific. The ethnicity issue is not developed in the paper unless I miss a specific section. Other comments could be more appropriate here.
	- The introduction should be revised so that the hypothesis and the objective are clearly understandable. My suggestion is that some parts of the Introduction could be moved to the Discussion (which could be improved too by following Authors' instructions and also see below) such as lines 1-29 page 2. More important, its organization should be clearer (suggestion among others: 1. Obesity trends and variations across social classes in UK; 2. Available information based on limited indicators (and specifying why they are limited); 3. Interest of a description combining extended social indicators and geographical parameters) along with important concepts well defined (inequality, insecurity).
	- Methods are too long since unnecessary explanations are provided. I wonder whether this is adapted to the BMJ Open readers. Anyway, some of these sections could also be moved to the discussion (lines 17-26 and lines 47- 56 page 5).
	- The Discussion could be revised following the Authors' instructions and enriched as suggested above. In addition, the Author could add information about the consistency of his conclusions with estimations using the NS-SEC indicators, which partially overlap. Another issue is the geographical level analyzed here which could be questionable since a large literature also exists about smaller levels, for instance, close neighborhood. The text from Page 9 Line 17 to page 10 line 19 is difficult to follow and some details are not necessary.
	- Tables 1-1 to 1-7: for each Savage category, provide prominent examples as footnotes. Prevalence could be additionally presented in figures to help understanding (and Tables as supplemental material available online). Actually is a map doable based on these results?
	Table 2 is cut. Footnotes should be added to specify what models 1, 2 and 3 were.

VERSION 1 – AUTHOR RESPONSE

Reviewer Cameron

More information is given about how this social class categories are arrived at by Savage et al (2013) in the Methods section, and what the Z scores of the distributions of the seven social classes by local authority mean. Latent class analysis is used for clustering of six continuous variables: mean status scores of contacts, total number of contacts, highbrow cultural capital, emerging cultural capital, income and assets. Seven social class clusters were identified: elite, who were 21.8% of the total survey sample, established middle-class (43.3%), technical middle-class (9.5%), new affluent workers

(5.8%), traditional working class (1.6%), emergent service sector (17.3%), and precariat (0.7%). The social class map of Savage et al expresses the proportion of each class in the sample by local authority as a Z score from the British average of the proportion of that class. This is given in the paper by Savage et al (2013), but a shortened explanation is useful and is now incorporated in the paper.

It was impossible to control for gender, as neither the social class mapping undertaken by Savage et al (21), nor the model-based estimates of adult obesity rates were available by gender at local authority level. I inquired of the modeller, Peter Scarborough to see if gender-specific disaggregations were available by local authority to be informed that the commission did not require it, nor had it been undertaken. The analysis was carried out at the local authority level because both obesity and social class data were available at this level; more fine-grained social class data by the schema used, were not available. While there are socio-economic status differences in obesity rates by gender, gender differences in obesity rates by indices of insecurity have not been identified. The distinction between inequality and insecurity is highlighted in the title, and clarified by defining the terms, in the introduction. It is important that the distinction between one the other be clear: while they may overlap, they represent different phenomena.

Reviewer Castetbon

The title now includes the type of study undertaken. Since ethnicity is not developed the paper, this has been withdrawn from the "strengths and limitations" section, and this section has been developed in a more appropriate way.

The Introduction has been revised such that the hypothesis and objective is more clearly understandable. Parts of the introduction have been moved the discussion section, and organised according to the principles specified by this reviewer. Obesity trends and variations across social classes in the UK are described, available information based on limited indicators is given. The purpose of the study, to describe obesity rates by both inequality and insecurity, is clearly stated. Both inequality and insecurity are defined.

Unnecessary explanations have been removed from the methods section, with appropriate information being moved to the discussion as recommended.

The discussion has been enriched, and information about the consistency of conclusions with estimations using the NS-SEC indicators now being given. The geographical level is set by the level of reporting of the new social class according to Savage et al (2013), and an analysis at more local levels, such as close neighbourhood, could not be undertaken.

Text from page 9, line 17 to page 10 line 19 has been edited down, removing unnecessary detail.

Tables 1-1 to 1-7 have footnotes added to them, with representative professions among the seven social classes. These tables have now been turned into supplemental material to be available online, and a new Figure 1 has been drawn and added. This summarises all of the data presented in Table 1. Regrettably, a map cannot easily be drawn based on these results.

Table 2 has been adjusted so that it no longer appears to be cut. Footnotes have been added to specify what models 1, 2 and 3 are.

I thank the reviewers for their very helpful advice.

VERSION 2 – REVIEW

REVIEWER	Noel Cameron Centre for Global Health and Human Development School of Sport, Exerise and Health Sciences Loughborough University UK
REVIEW RETURNED	18-Mar-2014

GENERAL COMMENTS	I remain concerned about the lack of analysis by gender. The wording in the strengths and limitations scetion - "the results did not disaggregate by gender" - implies that gender was found to be not significant rather than that it was not possible to test for gender effects. For clarity I would suggest making this point obvious.

REVIEWER	CASTETBON Katia Institut de veille sanitaire
REVIEW RETURNED	12-Mar-2014

GENERAL COMMENTS	Part of my comments have been taken into account, as stated by the author. However, to my point of view, changes regarding the Methods and the Discussion are actually limited. I still wonder whether the paper fits with the standard of the BMJ Open review, which can be decided by the Editor.

VERSION 2 – AUTHOR RESPONSE

I have made the minor revisions as suggested by the reviewers, and now hope that this manuscript is acceptable for publication.

I have responded to reviewer Cameron by adopting the wording 'it was not possible to test for gender effects' in both the strengths and limitation section, and in the methods section

I have responded to reviewer Castetbon by cutting 4 sentences from the methods, and 17 lines from the dicussion, without cutting into the additional detail requested by reviewer Cameron in the first round of reviewing. This now significantly tightens up both sections, and I am grateful for her careful eye.