

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Socio-economic inequality in salt intake in Britain 10 years after a national salt reduction programme.
<b>AUTHORS</b>	Ji, Chen; Cappuccio, Francesco

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Linda Cobiac University of Queensland, Australia
<b>REVIEW RETURNED</b>	27-May-2014

<b>GENERAL COMMENTS</b>	<p>I have no experience in using the specific Bayesian geo-additive models used in this paper, and cannot therefore provide good judgement on the appropriateness or application of these methods.</p> <p>This is an interesting and well-written paper. It is an important topic: sodium consumption exceeds recommended levels in almost all countries of the world, and there is enormous potential for reducing global cardiovascular disease burden. The UK is one of only a few countries to implement population-wide strategies to address sodium intake. The combination of intervention strategies has been successful in reducing population sodium level over a 10 year time period. This study, although having to rely on analyses of less than ideal data (e.g. food diaries rather than urinary sodium excretion measures; excluding ethnic minority groups), provides insight on how socio-economic inequalities in sodium intake have (not) changed over this same time period.</p> <p>Some minor comments.  P4, first sentence: There may be a more up-to-date reference from the 2010 GBD  P4, line 14-15: "However, the majority of events..." Does this statement need a reference?  P5, line 35: "low-and-income" Does this need a 'middle'?  P11, line 20-25: could the salt reduction reflect some (albeit small) behavioural changes in food choices, as well as food reformulation?  P11, line 30-31: "The diet of socio-economically disadvantaged groups..." This is a rather sweeping statement. Maybe consider re-wording or giving a reference.  P11, line 33: should 'revert' be 'reverse'? This sentence is slightly odd grammatically.</p>
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<b>REVIEWER</b>	Jean Adams Newcastle University, UK
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REVIEW RETURNED	11-Jun-2014
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GENERAL COMMENTS	<p>This is an interesting analysis of socio-economic inequalities in salt intake in Great Britain from the 2008-11 National Diet and Nutrition Survey, comparing results to previous data from 2000-01 to explore change over time. The topic is very valuable and the authors have identified the potential of these data that have been relatively underexploited to date. There are a number of limitations in data analysis and interpretation that could be relatively easily addressed to improve the quality of the manuscript. However, given that a key aim of the paper is to explore changes in inequalities over time, and urinary sodium data (comparable to that used in the 2000-01 analysis) is not yet available for the 2008-11 cohort, I would suggest that the authors be encouraged to wait until this data is available in order to perform a true like-for-like comparison.</p> <p><b>Substantive comments</b></p> <p>I would have like to see more information in the introduction concerning the previous analysis – particularly, what measure of SEP was used and whether there were any differences in inequalities seen between different measures of SEP (as was found in the current analysis).</p> <p>The introduction and discussion lack strong theoretical grounding in terms of ‘intervention generated inequalities’. Reviews and concept pieces have identified that some types interventions are likely to widen, and others narrow, inequalities. I’d like to see the UK salt reduction programme put in the context of this theory and evidence and so the case made for why the programme might, or might not, have reduced inequalities. Would also be helpful to pick up on this in the discussion. The current literature would suggest that this sort of population level intervention should reduce inequalities. So the fact that it doesn’t appear to have done needs explored.</p> <p>The introduction, discussion, and the approach to analysis do not to take into account the literature on how to measure inequalities in health. The measurement approach can have important implications for conclusions drawn. For example, the current work explores relative differences between socio-economic groups – and finds an increase in inequality. But I suspect (although would like to see the data presented here) that if absolute differences were explored, there may have been a decrease in inequality. There is a useful report on this here:  <a href="http://www.scotpho.org.uk/downloads/scotphoreports/scotpho071009_measuringinequalities_rep.pdf">http://www.scotpho.org.uk/downloads/scotphoreports/scotpho071009_measuringinequalities_rep.pdf</a></p> <p>I wasn’t sure why non-white and Northern Irish people couldn’t have been included in the overall analysis – even if not in some specific analyses.</p> <p>The “range of important covariates” adjusted for should be detailed. There is lots of detail in the methods section concerning variables that it is not obvious were included in the analysis; and it would be valuable to justify why the covariates included were considered to be potential confounders. For example, how might blood pressure confound the SEP-salt intake relationship?</p> <p>I disagree with the conclusion that “social inequalities in salt consumption have remained”. This is only the case for when SEP is measured by education, and not by occupation. More consideration and exploration of why the difference by different measures of SEP would be useful. Also, from Table 3, the trend in sodium consumption</p>
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	<p>by education is not straightforwardly progressive across educational groups and this should be considered further.</p> <p>It seems possible that there are socio-economic differences in how discretionary salt has responded to changes in prepared food salt. So less affluent individuals may have added more discretionary salt than previously, whereas more affluent may have not. Furthermore, the exclusion of discretionary salt from the 2008-11 data set means that like is not being compared with like when exploring differences in inequalities over time. For this reason, I think it there is a strong justification for waiting until urinary sodium data are available from the rolling programme NDNS before attempting the analyses reported here. Reference is made to a study that reports urinary sodium data from 2011 (ref 21) – if those authors have the data, why not the authors of the current manuscript?</p> <p>It is a massive oversimplification to say “the diet of socio-economically disadvantaged groups is made of low-quality, salt-dense, high-fat, high-calorie unhealthy cheap foods”. This statement should be unpacked and referenced. There are certainly socio-economic differences in dietary quality. But this statement is caricature.</p> <p>The relative differences in salt intake reported – 5-6% - seem quite small. Why are they smaller than the 9% difference reported in Australia? How important are such small differences?</p> <p>The conclusion could have been stated without doing the work reported. It would be better formulated as a concise response to the aims.</p> <p><b>Minor comments/typos (line numbers as generated by ScholarOne)</b></p> <p>P4, line 47-8. I think there is a typo here and 15% should be 25%? The figure 25% is used throughout the rest of the manuscript.</p> <p>P5, line 24. I think “poor diet” is preferable to “bad diet” – dietary quality is not dichotomous.</p> <p>P5, line 33-7. I didn't understand this sentence. There are words missing, but I wasn't even sure what the intended meaning was.</p> <p>P6, line 23. Please explain why some people completed 3-day, and others 4-day food diary.</p> <p>P7, line 10. There are better references for NS-SEC than NDNS documentation. Please cite the report describing the development of the tool.</p> <p>P7, line 38-9. I presume the outcome of interest was <i>daily</i> sodium intake?</p> <p>P8, line 6-7. Please explain the relevance of “UK Borders”.</p> <p>P8, line 29. What's the rationale for <math>\alpha=0.1</math> in models? Is this standard?</p> <p>P8, line 55. What is an “observatory map”?</p> <p>P9, line22-23. I don't understand what is implied by “5-category factors”.</p> <p>P11, lines 8-9 and 19. Why are the figures of 1.4g and 0.9g reduction in population salt intake not the same? Aren't these figures calculated from the same data. I think perhaps you need to clarify throughout that the figures reported from the current data</p>
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	are not total salt intake?
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## VERSION 1 – AUTHOR RESPONSE

Reviewer Name Linda Cobiac

Institution and Country University of Queensland, Australia

Please state any competing interests or state 'None declared': None declared

I have no experience in using the specific Bayesian geo-additive models used in this paper, and cannot therefore provide good judgement on the appropriateness or application of these methods. This is an interesting and well-written paper.

It is an important topic: sodium consumption exceeds recommended levels in almost all countries of the world, and there is enormous potential for reducing global cardiovascular disease burden.

The UK is one of only a few countries to implement population-wide strategies to address sodium intake.

The combination of intervention strategies has been successful in reducing population sodium level over a 10 year time period.

This study, although having to rely on analyses of less than ideal data (e.g. food diaries rather than urinary sodium excretion measures; excluding ethnic minority groups), provides insight on how socio-economic inequalities in sodium intake have (not) changed over this same time period.

Some minor comments.

QUESTION: P4, first sentence: There may be a more up-to-date reference from the 2010 GBD

ANSWER: Thank you. We have replaced the previous reference with Lozano R et al. Lancet 2012;380:2095-128

Q: P4, line 14-15: "However, the majority of events..." Does this statement need a reference?

A: Thank you. We have now added Lewington et al Lancet 2002;360:1903-13

Q. P5, line 35: "low-and-income" Does this need a 'middle'?

A. Thank you. The reviewer is correct. However, we have removed that sentence as out of place in the context.

Q. P11, line 20-25: could the salt reduction reflect some (albeit small) behavioural changes in food choices, as well as food reformulation?

A. We feel this is unlikely as recent evidence from the analysis of data for take-home food and beverage purchases from British households in 2010 indicates that shopping baskets of higher SES groups are healthier (proportionally more purchasing of fiber, protein and total sugars and less sodium) than those of lower SES groups (Pechey R et al. 2013).

Q. P11, line 30-31: "The diet of socio-economically disadvantaged groups..." This is a rather sweeping statement. Maybe consider re-wording or giving a reference.

A. Thank you. The sentence briefly but, in our view, effectively, summarises a wealth of evidence supporting our statement. We now add 8 references, a portion of the extensive literature around the subject.

Q. P11, line 33: should 'revert' be 'reverse'? This sentence is slightly odd grammatically.

A. Thank you. It should have been 'reverse'. Amended.

Reviewer Name Jean Adams

Institution and Country Newcastle University, UK

Please state any competing interests or state 'None declared': None declared

Q. This is an interesting analysis of socio-economic inequalities in salt intake in Great Britain from the 2008-11 National Diet and Nutrition Survey, comparing results to previous data from 2000-01 to explore change over time. The topic is very valuable and the authors have identified the potential of these data that have been relatively underexploited to date. There are a number of limitations in data analysis and interpretation that could be relatively easily addressed to improve the quality of the manuscript. However, given that a key aim of the paper is to explore changes in inequalities over time, and urinary sodium data (comparable to that used in the 2000-01 analysis) is not yet available for the 2008-11 cohort, I would suggest that the authors be encouraged to wait until this data is available in order to perform a true like-for-like comparison.

A. We thank the reviewer for her thoughtful and useful comments that we have tried and take on board whenever possible. One aspect that is raised here and later, however, needs clarification. We had raised the query of the urine data to the UK Data Service before the start of our analysis and learned that "the data will not be available until after the year 4 report has been published when year's 1-4 will be deposited" (i.e. 2008-2012). However, the complete urine data are still not available in the UK Data Service database (checked 24/06/2014). Whilst in the 2000-1 NDNS 24h urine collections were carried out in the whole national sample of adults, in the 2008-11 three cohorts were assessed with 24h urine collections, including participants aged four to 18 years and 65 years and over in Years 1 to 4 combined of the NDNS Rolling Programme (RP). The RP data recently published<sup>1</sup> add to previous publications 1,2,3,4 by estimating the mean and population distribution of 24-hour salt intake (g per day) in the UK among those aged four to 18 years and 65 years and over, based on analysis of sodium in 24-hour urine collections<sup>2</sup>. Results for adults aged 19 to 64 years (the objective of our analysis) are not presented in this publication because results for this age group, based on data collected separately and over a shorter time period in England (2011) and Scotland (2009/10), were published in 2012 and 2011, respectively<sup>3-4</sup>. We shall rephrase our paper by making absolutely clear that the comparison of the present paper only refers to salt intake assessed by dietary means which realistically does not include the proportion of discretionary salt use.

1. 14/05/2014: <https://www.gov.uk/government/publications/national-diet-and-nutrition-survey-results-from-years-1-to-4-combined-of-the-rolling-programme-for-2008-and-2009-to-2011-and-2012>)

2. Chapter 7 in Public Health England & Food Standard Agency. National Diet and Nutrition Survey. Results from Years 1, 2, 3 and 4 (combined) of the Rolling Programme (2008/2009 – 2011/2012). A survey carried out on behalf of Public Health England and the Food Standards Agency. Bates B, Lennox A, Prentice A, Bates C, Page P, Nicholson S and Swan G eds., 2014; pp. 1-158).

3. National Diet and Nutrition Survey - Assessment of dietary sodium in adults (aged 19 to 64 years) in England, 2011 report.  
[https://www.gov.uk/government/uploads/system/uploads/attachment\\_data/file/127916/Sodium-Survey-England-2011\\_Text\\_to-DH\\_FINAL1.pdf](https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/127916/Sodium-Survey-England-2011_Text_to-DH_FINAL1.pdf) (accessed 25/06/14).

4. A survey of 24-hour urinary sodium excretion in a representative sample of the Scottish population as a measure of salt intake, April 2011;  
<http://food.gov.uk/scotland/researchscot/scotlandresearch/ScotlandProjectList/s14047/> (accessed 25/06/14).

Substantive comments

Q. I would have like to see more information in the introduction concerning the previous analysis –

particularly, what measure of SEP was used and whether there were any differences in inequalities seen between different measures of SEP (as was found in the current analysis).

A. We have added a sentence in the Background to expand on the results of the previous analysis. "In 2,105 men and women aged 19-64 years, salt consumption was assessed using both 7-day dietary records and 24h urinary sodium excretion. Socio-economic position was defined both on head of household occupation and on participant's educational attainment. Bayesian geo-additive models via Markov Chain Monte Carlo simulations were used to test the independent associations accounting for linear and non-linear effects and spatial variations. Both dietary and total salt consumption were higher in Scotland and in lower socio-economic groups, whether assessed by occupation or by educational attainment (with difference estimates varying from 4% to 9%)."

Q. The introduction and discussion lack strong theoretical grounding in terms of 'intervention generated inequalities'. Reviews and concept pieces have identified that some types interventions are likely to widen, and others narrow, inequalities. I'd like to see the UK salt reduction programme put in the context of this theory and evidence and so the case made for why the programme might, or might not, have reduced inequalities. Would also be helpful to pick up on this in the discussion. The current literature would suggest that this sort of population level intervention should reduce inequalities. So the fact that it doesn't appear to have done needs explored.

A. Thank you. We agree with the reviewer that the current theories would suggest a reduction in SES inequalities with population level interventions. It is why in this context we find our results of interest. We have expanded in Background.

"In general, 'downstream' preventive interventions with focus on individual behavioural changes are more likely to increase health inequalities than 'upstream' social or policy interventions<sup>®</sup>. Amongst the former, media campaigns may be particularly likely to increase inequalities whilst the latter tend to reduce health inequalities, as they are usually 'structural'<sup>27</sup>."

"Fifth, the UK salt reduction programme started with three waves of media campaigns to increase awareness and change behaviour. The engagement with industry, which included target settings and food reformulation, was implemented later and its effects might not have had enough time to impact on social inequalities."

Q. The introduction, discussion, and the approach to analysis do not take into account the literature on how to measure inequalities in health. The measurement approach can have important implications for conclusions drawn. For example, the current work explores relative differences between socio-economic groups – and finds an increase in inequality. But I suspect (although would like to see the data presented here) that if absolute differences were explored, there may have been a decrease in inequality. There is a useful report on this here:

[http://www.scotpho.org.uk/downloads/scotphoreports/scotpho071009\\_measuringinequalities\\_rep.pdf](http://www.scotpho.org.uk/downloads/scotphoreports/scotpho071009_measuringinequalities_rep.pdf)

A. Our conclusions do not suggest an 'increase' in inequalities but - rather - that inequalities are still detectable after the 8-year programme. We have now re-phrased our manuscripts in places to make this absolutely clear. We have also extended our Introduction with basic reference to measures of inequalities in health as suggested<sup>5-8</sup>.

5. ScotPHO. Measuring socio-economic inequalities in health: a practical guide, 2007; pp. 1-7

6. Mackenbach JP and Kunst A. Measuring the magnitude of socio-economic inequalities in health: and overview of available measures illustrated with two examples from Europe. *Social Science Medicine*. 1997;44(6): 757-771

7. Wagstaff A, Paci P, Van Doorslaer E. On the measurement of inequalities in health. *Social Science and Medicine*. 1991; 33(5): 545-557

8. Gakidou EE, Murray CJL, Frenk J. Defining and measuring health inequality: an approach based on the distribution on health expectancy. *Bull WHO* 2000; 78:42-54



Q. I wasn't sure why non-white and Northern Irish people couldn't have been included in the overall analysis – even if not in some specific analyses.

A. The number of non-white participants in the survey is small. In the 2008-2011 data, only 10% (n=117) of the adults sample come from minor ethnic background, which includes Asian, Black, Chinese, mixed and other. Considering the potential heterogeneity between ethnic groups and, in particular, when compared by region, the number of ethnic groups in each region is extremely small, which, if the participants are included in the analysis, may lead to unreliable conclusions. Northern Ireland was excluded from the analysis because Bayesian geo-additive models are best applied when a 'connected' map is available. That is, each region in the map is expected to be connected with one or more other regions so that a network of neighbouring regions can be defined by the map to provide the estimation basis for the spatial dependence. In our analysis, however, Northern Ireland (NI) is separated from mainland Britain. Unfortunately, there is no widely agreed criterion to define which regions can be considered as the neighbours of NI. Hence, the NI data were not used (see Geographical boundaries).

Q. The “range of important covariates” adjusted for should be detailed. There is lots of detail in the methods section concerning variables that it is not obvious were included in the analysis; and it would be valuable to justify why the covariates included were considered to be potential confounders. For example, how might blood pressure confound the SEP-salt intake relationship?

A. The covariates included in the models are the conventional participants' socio-demographic factors. We used the same covariates as those used in our previous analysis, which enabled us to make a like-for-like comparison in terms of salt intake and model generalizability. They are all listed in Table 3 with their coefficients.

Q. I disagree with the conclusion that “social inequalities in salt consumption have remained”. This is only the case for when SEP is measured by education, and not by occupation. More consideration and exploration of why the difference by different measures of SEP would be useful. Also, from Table 3, the trend in sodium consumption by education is not straightforwardly progressive across educational groups and this should be considered further.

A. We have removed a sentence in abstract and re-phrased Key messages in box

Q. It seems possible that there are socio-economic differences in how discretionary salt has responded to changes in prepared food salt. So less affluent individuals may have added more discretionary salt than previously, whereas more affluent may have not. Furthermore, the exclusion of discretionary salt from the 2008-11 data set means that like is not being compared with like when exploring differences in inequalities over time. For this reason, I think it there is a strong justification for waiting until urinary sodium data are available from the rolling programme NDNS before attempting the analyses reported here.

A. Please see previous response of data availability.

Q. Reference is made to a study that reports urinary sodium data from 2011 (ref 21) – if those authors have the data, why not the authors of the current manuscript?

A. The urinary sodium data quoted in He et al (2014) were extracted from the NDNS England Sodium Survey 2011

([https://www.gov.uk/government/uploads/system/uploads/attachment\\_data/file/213420/Sodium-Survey-England-2011\\_Text\\_to-DH\\_FINAL1.pdf](https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/213420/Sodium-Survey-England-2011_Text_to-DH_FINAL1.pdf)), which collected data from two sources: the core NDNS sample and a “Sodium Boost” study. Although both urine data collection followed the NDNS protocol, the survey is different from the NDNS and the data cannot be used in our analysis.

Q. It is a massive oversimplification to say “the diet of socio-economically disadvantaged groups is made of low-quality, salt-dense, high-fat, high-calorie unhealthy cheap foods”. This statement should

be unpacked and referenced. There are certainly socio-economic differences in dietary quality. But this statement is caricature.

A. Whilst the statement may seem a caricature, sufficient evidence is available in the literature to support an oversimplified sentence. We now provide detailed references to support our statement.

Rao M, Afshin A, Singh G, Mozaffarian D. Do healthier foods and diet patterns cost more than less healthy options? A systematic review and meta-analysis. *BMJ Open* 2013; 3(12):e004277.

Banks J, Marmot M, Oldfield Z, Smith JP. Disease and disadvantage in the United States and in England. *JAMA* 2006; 295(17):2037-2045.

Kant AK, Graubard BI. Secular trends in the association of socio-economic position with self-reported dietary attributes and biomarkers in the US population: National Health and Nutrition Examination Survey (NHANES) 1971-1975 to NHANES 1999-2002. *Public Health Nutr* 2007; 10(2):158-167.

Darmon N, Drewnowski A. Does social class predict diet quality? *Am J Clin Nutr* 2008; 87(5):1107-1117.

Jetter KM, Cassady DL. The availability and cost of healthier food alternatives. *Am J Prev Med* 2006; 30(1):38-44.

McDermott AJ, Stephens MB. Cost of eating: whole foods versus convenience foods in a low-income model. *Fam Med* 2010; 42(4):280-284.

Rehm CD, Monsivais P, Drewnowski A. The quality and monetary value of diets consumed by adults in the United States. *Am J Clin Nutr* 2011; 94(5):1333-1339.

Q. The relative differences in salt intake reported – 5-6% - seem quite small. Why are they smaller than the 9% difference reported in Australia? How important are such small differences?

A. SES difference in salt intake in Britain in 2000-1 varied from 4% (24h urinary sodium in manual occupation) to 9% (24h urinary sodium in low educational attainment). In the present survey participants with low educational attainment had 5.7% higher salt intake. 90% credible intervals suggest these estimates are compatible with each other. The results from Australia, whilst qualitatively concordant with our results, cannot be directly compared to our study because: (a) they studies 2-16 years old children; (b) salt intake patterns and policy interventions differ substantially between the two countries; (c) measures of SES refer to parents (parental income) and primary carers (educational attainment). Nevertheless, 9% average differences are still compatible with our own results. As for the importance of such differences, the 8-year programme in the UK has achieved a 15% reduction in average population salt intake across the board. A 5% difference in salt intake between more and less affluent groups would be equivalent to a third of this effect.

Q. The conclusion could have been stated without doing the work reported. It would be better formulated as a concise response to the aims.

A. We have now added a sentence in Abstract and Conclusions

“Social inequalities in salt intake have not seen a reduction following the national salt reduction programme and still explain more than 5% of salt intake between more and less affluent groups.”

Minor comments/tips (line numbers as generated by ScholarOne)

Q. P4, line 47-8. I think there is a typo here and 15% should be 25%? A. The figure 25% is used throughout the rest of the manuscript.

Thank you for pointing this out. 15% is actually right. The rest of the sodium is naturally presented in



food. But the “25%” in line 17, P10 (Strengths and limitations) is a typo and should be read as 15%.

Q. P5, line 24. I think “poor diet” is preferable to “bad diet” – dietary quality is not dichotomous.

A. Agreed – changed as suggested

Q. P5, line 33-7. I didn’t understand this sentence. There are words missing, but I wasn’t even sure what the intended meaning was.

A. We are not sure what the reviewer refers to – the text in this section has been altered substantially. We hope the sentences are now clearer.

Q. P6, line 23. Please explain why some people completed 3-day, and others 4-day food diary.

A. In the NDNS 2008-11, participants who attended the interview and completed at least three days of the food diary were considered as “fully productive” and were invited to continue the survey. In the entire “fully productive” population (n=3,073, including children), only 2% (n=53) provided a 3-day food diary. This information can be found in the “Methodology” section of the “Bates B, Lennox A, Prentice A, Bates C, Swan G. National Dietary and Nutrition Survey - Headline results from Years 1, 2 and 3 (combined) of the Rolling Programme (2008/2009 – 2010/11). Department of Health, 2012.” (Available at <http://www.natcen.ac.uk/media/175123/national-diet-and-nutrition-survey-years-1-2-and-3.pdf>). Therefore, we included all “fully productive” participants in our analysis.

Q. P7, line 10. There are better references for NS-SEC than NDNS documentation. Please cite the report describing the development of the tool.

A. Thank you. We have added Rose D & Pevalin DJ. The National Statistics Socio-economic Classification: origins, development and use. Palgrave Macmillan, Basingstoke, 2005; pp. 1-120

Q. P7, line 38-9. I presume the outcome of interest was daily sodium intake?

A. Thank you – added a short line to make it clear.

Q. P8, line 6-7. Please explain the relevance of “UK Borders”.

A. Thanks for pointing out this typo. This should be UKBORDERS which provides the geographical data of Britain. It now has become part of UK Data Service Census Support.

Q. P8, line 29. What’s the rationale for alpha=0.1 in models? Is this standard?

A. The studies using Bayesian geo-additive models usually employ 80% or 90% credible levels. We used 90% in our last paper on the NDNS 2000-01. Hence, this level was used in this analysis for a like-for-like comparison.

Q. P8, line 55. What is an “observatory map”?

A. Observatory map is a simple graphical description presenting the sodium level by region on a map. The statistics were unadjusted.

Q. P9, line22-23. I don’t understand what is implied by “5-category factors”.

A. This is another occupation factor which extended the 3-category factor to 5 levels, which described the occupation status with greater detail. We have now changes ‘factor’ into ‘occupation’.

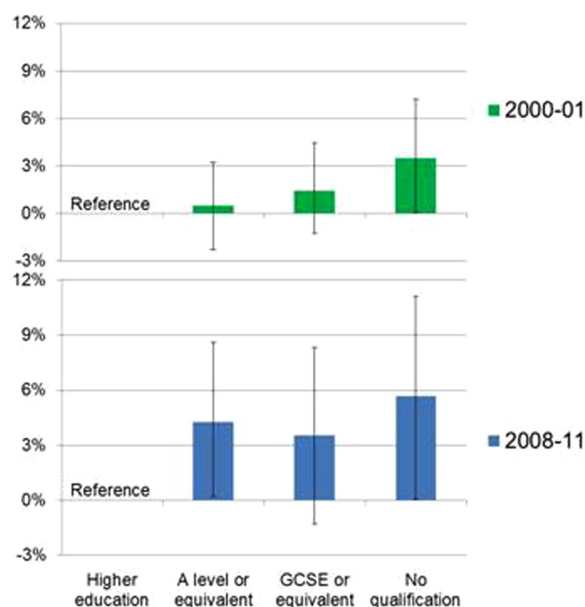
Q. P11, lines 8-9 and 19. Why are the figures of 1.4g and 0.9g reduction in population salt intake not the same? Aren’t these figures calculated from the same data. I think perhaps you need to clarify throughout that the figures reported from the current data are not total salt intake?

A. 1.4g is TOTAL salt reduction, 0.9g refers to DIETARY salt reduction (not including DISCRETIONARY salt consumption). The data differ from our data mainly in three aspects: 1) the estimation was made upon England data only, while ours was based on England, Wales and Scotland samples; 2) the salt intake was derived from urinary sodium excretion, while ours were estimated

using dietary sodium intake. Regardless of other differences, discretionary salt account for most of the gap between dietary (0.9g) and urinary sodium (1.4g) measurements; 3) The urine data in 2011 were actually obtained from the NDNS England Sodium Survey 2011, which collected data from two sources: (a) the core NDNS sample (N=48) and (b) a “Sodium Boost” study (N=552). The urine samples from the core NDNS only accounts for a small proportion of the NDNS population.

## Correction

Ji C, Cappuccio FP. Socioeconomic inequality in salt intake in Britain 10 years after a national salt reduction programme. *BMJ Open* 2014;4:e005683. The authors have become aware that during the submission process the incorrect figure 2 was uploaded and subsequently published. The figure does not match the data reported in table 3, which is indeed correct and it is referred to for the results of the study and discussion. Whilst the mistake is uninfluential, the figure may be misleading for some. The amended figure is below. The legend is unaffected.



*BMJ Open* 2014;4:e005683corr1. doi:10.1136/bmjopen-2014-005683corr1