PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Global, regional, and national sodium intakes in 1990 and 2010: a systematic analysis of 24-hour urinary sodium excretion and dietary surveys worldwide
AUTHORS	Powles, John; Fahimi, Saman; Micha, Renata; Khatibzadeh, Shahab; Shi, Peilin; Ezzati, Majid; Engell, Rebecca; Lim, Stephen; Danaei, Goodarz; Mozaffarian, Dariush

VERSION 1 - REVIEW

REVIEWER	Colin Mathers Coordinator, Mortality and Burden of Disease Unit World Health Organization Geneva, Switzerland I have no conflicts of interest.
REVIEW RETURNED	17-Sep-2013

THE STUDY	There needs to be a little more detail on the statistical model used, this could be via reference to other published work. See my comments to author. I don't see this as a major issue, and the paper is a very important contribution to the public health literature that should be accepted with a little revision.
GENERAL COMMENTS	This study estimates mean sodum intakes in adults in 1990 and 2010 at global, regional and national levels. The study has carried out a well documented and extensive literature and data search which collected data from 142 surveys of 24-hour sodium excretion and 103 dietary surveys allowing estimation of dietary sodium. The study prioritized urinary sodium as the primary measure and used a subset of surveys which collected both types of data for individuals to cross-walk between the dietary and urinary measures. The advantages of this approach are well described, as are some adjustments for potential bias, and discussion of other potential biases and their potential magnitude. The results are well presented and the discussion and conclusions appropriate. My only concern with the paper relates to the statistical methods used, where the description is somewhat vague and some clarifications needed. With minor revision, the paper is well worth publication, and is of considerable public health importance given current international efforts to establish non-communicable disease targets, including a target for sodium intake reduction. Some detailed comments follow. P5.line 8. Is gastric cancer a commonly understood term for stomach cancer? I believe the latter term is more in line with cancer classification terminology, but may be have spent too long buried in

	disease coding discussions. P6. Line 36. I think STEPS is the correct acronym, not STEP P7. Line 47. I am not an expert on dietary analysis, but standardization of intakes to 2000 kcal per day seems too low for many developed countries today. Is one global figure really appropriate? P8. Line 21. Little detail is given here of the Bayesian hierarchical model, although it is said that the authors developed it. However, the methods annex says that this model was DISMOD-III (without further reference or explanation). I assume this is the DISMOD tool used in GBD 2010 study, which I understood was being referred to as DISMOD-MR rather than DISMOD-III. There needs to be some more detail on the modelling methods and assumptions as well as a reference to documentation of DISMOD-MR. The authors should either clarify or reference the model assumptions and methods incorporated in DISMOD-III. They do mention that a cubic spline model was used for estimating age patterns, was a similar approach taken for modelling of time trends? P48, line 24. This sentence seems to suggest tht the standard deviations (of the mean effect sizes in the table above?) were estimated separately outside DISMOD using another regression model. This seem quite odd and deserves some more explanation, or rewording if I am misunderstanding it.

REVIEWER	Kenji Shibuya Professor and Chair Department of Global Health Policy, Graduate School of Medicine, University of Tokyo I am a member of the GBD core group
REVIEW RETURNED	19-Sep-2013

GENERAL COMMENTS	Overall comment Sodium is among the leading risk factors for disease burden across the world. I would like to congratulate the authors for the heroic effort they have made to provide a systematic and comprehensive assessment of trends in sodium intake, which incorporates both new data and statistical methods to enhance comparability across countries and over time. The manuscript is well written and the analysis is solid, but it is possible that one step of their analysis needs to be better described and possibly modified, and their discussion of certain aspects of the results needs more caveats. Specific comments Page 8, Figure 2. There seem to be 8 age and sex strata that follow their own very

	clear curve that might represent some kind of stratifying or confounding variable. If this variable could be identified and included in the regression model it would potentially significantly increase the R-squared statistic for this model. The authors should investigate these 8 strata and see if they arise from a specific study, region or period, and consider developing a slightly more detailed regression model based on this. It is possible that this would improve overall accuracy of the model through better development of the crosswalk regression. Page 8, Bayesian hierarchical model There is no test of predictive validity in the present study. Thus, I cannot judge the performance of the present approach. If possible, it would be useful to see a sub-analysis using only countries and regions with large amounts of available data, perhaps presenting the results of some form of cross-validation process. For example, the countries with large amounts of available data could be divided into training and validation datasets, and the predictive accuracy of the model shown for this restricted data set. Page 10, Results The authors did a great job providing relative uncertainties in Figure 3. However, as indicated in Table 1, the data sources are highly skewed to high- and middle- income countries where examination surveys are in place. There were no data available for over 120 countries. The current approach is a hybrid of empirical data, models and extrapolations. The major drawback of this approach is that the prediction for countries with limited data points tends to be derived from covariates or extrapolations from neighbouring countries, which is arbitrary and thus makes the assessment of estimation less compelling than that of, for example, child mortality series. Very few surveys are available in sub-Saharan African and South and Southeast Asian regions. As there is limited correlation between sodium and national income, I wonder how valid the current results, in particular on trends, would be in these regions where the next epidemic of stroke will take place. From the figure in the web appendix, it appears that for some of these regions with sparse data, the prior and the posterior distribution are very similar. This may indicate that the choice of prior is essentially driving the results for these regions. The authors should offer caveats in the text indicating the extent to which the results are driven by the priors. Supplemental material page 2, factor analysis More information is necessary about the factor analysis used to derive the food components of the Bayesian model. This factor analysis appears to be very important for driving country-specific outcomes (see e.g. the fixed effects shown for the region- and sex-specific model outcomes in the following figure). However, the

	method for this factor analysis is not clear, the text and Table S1 seem to present different results, and the description appears to confuse factor analysis and principal components. The authors need to clarify this analysis by: 1. Indicating whether the loadings shown in Table S1 are from the eigenvectors of a principal component analysis or from a factor analysis 2. If the loadings in Table S1 are derived from a factor analysis the authors should describe the rotation method used in the text on page 2 3. If not from a factor analysis, the authors should not refer to this section of their analysis as a “factor analysis” but as “dimension reduction through principal component analysis” 4. If factor analysis was used, indicate whether they used a principal factor extraction method or a maximum likelihood method The authors also should present the eigenvalue of each factor, variance explained by each factor, and total variance explained by the four factors. The authors have used the Kaiser criterion (all factors with eigenvalues > 1) to select the number of factors to include. From looking at Table S1 and Table S2, however, it appears that the fourth factor may be unnecessary, and that even though its eigenvalue is greater than one it contributes very little to variance explained. The authors’ interpretation of the meaning of each factor/principal component also seems disputable. For example, component 1 appears to be a meat-and-alcohol factor, while component 2 is a contrast between saturated fats and vegetables/fibres. The authors state that “factor 4 has the highest loading on sugars, stimulants and saturated fats” but in fact the highest loading for sugars and stimulants is in factor 1, and the highest loading for saturated fats in factor 2. It appears that the text on page 2 does not match the factors reported in Table S1, and the authors need to clarify this. These factors also look as if they might represent regional dietary structures: component 1 may represent a western diet and component 4 an east Asian diet, for example. If so, there may be significant collinearity between the factors and the regional random effects: the authors should check this possibility by comparing factor values between regions or nations. Given the importance of the factor analysis of food inputs to calculation of sodium levels, this information needs to be clarified and, if necessary, the model should be rerun with a revised factor analysis.

REVIEWER	Thorhallur Ingi Halldorsson Associate professor at Faculty of Food Science & Nutrition University of Iceland, Reykjavik

	Iceland No competing interests
REVIEW RETURNED	19-Sep-2013

GENERAL COMMENTS	This is a well written and informative paper estimating sodium intakes between 1990-2010 in a 187 countries. Despite methodological challenges of estimating sodium intakes (in general) which is likely to result in substantial variation in quality between different studies, the results from this study are highly relevant from public health and policy point of view. I have only few minor remarks: **ABSTRACT** 1) It's quite confusing when you first state that sodium intake was estimated in adults in **1990** and 2010 (objectives) and then in the "Data sources and eligibility" you say that surveys conducted between **1980** and 2010 were used. This 1990 and 1980 issue needs further clarification in the abstract. 2) In the result section of the abstract you say that men had approx. 10% higher intake than women. Is that absolute value or is it accounted for by difference in weight (or energy intake). Men have absolute higher intake of sodium (an other nutrients) as men have higher body weight on average. If highlighted in the abstract I would say "As expected men had ~10% higher sodium …." (and throughout the manuscript). 3) Please state in the abstract what the recommended sodium intake is in g/d so the mean intakes can be compared to that number in the abstract. Also report % of countries above this recommendations (as done later in this paper). This is the main result from this study and it needs to be highlighted in the abstract (not just mentioned in the conclusion in the abstract) 4) Also highlight the % above recommendation in key messages **MAIN TEXT**

	5) Methods, page 6, line 16. It might be appropriate to add a reference to a suitable introductory book (or chapter) where hierarchical Bayesian imputation is described. 6) Methods, page 7, line 11 and lines 51-59. You mention differences in accuracy when relaying on 24h urine vs. dietary assessment methods. Concerning the dietary assessment methods a very large variability in accuracy for estimating sodium is expected. As an example FFQs can rank individual subjects according to intake accurately but absolute values are poorly determined. On the other hand diet records and recalls should be much more accurate on a group level (although records may influence subjects eating habits). If a range of dietary methods were available from one (or comparable) countries differences in estimates between methods could be examined and reported (is FFQs are suitable for inclusions ?). Limitations of using estimates from different dietary methods should at least be mentioned and acknowledged. 7) Methods, page 8, line. You mention here and in abstract non-urinary losses (sweat). Although only 10% loss can be expected on average (as you state) is it possible that variation between regions could be partly explained by non-urinary losses (when relaying on 24h urine). In summary is it possible that non-urinary losses are greater in Sub-Saharan Africa compared to Scandinavia (on average). 8) Results, page 10, line 1: Please state the mean number of subjects in each survey and range (for these 245 surveys), preferably for 24h urine and dietary surveys separately. Reporting the total number of subjects in all these 245 surveys might also be appropriate 9) Result, page 10, line 18: could you explain in slightly more detail what a “data point” refers to in your study.

	10) Result, page 10, line 44. Again I would add “as expected” when it comes to this gender difference 11) Result, page 11, line 6. What does “UI” stand for (has it been written out in full for the first time prior to this ?) 12) Results page 12, lines 15-21: When you say that 99.2% or the world’s population exceeds the WHO recommendation does that refer to that the average in 99.2% of the countries exceeds the recommendation?? It would also be informative if you could report the % of subjects in each country that exceed the recommendations based on all these surveys (even though the mean is above the variation between countries may differ and it is informative to know the % of subjects above/or below the recommendation in each country). 13) Discussion page 13, line 41: You say “Numerous other organizations have systematically reviewed the evidence and concluded that high intakes of sodium increase cardiovascular risk.11 27 30” Can you be more speck in terms of what “**high**” means here ? 14) Discussion, page 15, line 32. Using the word “optimal” here might be too strong.

VERSION 1 – AUTHOR RESPONSE

Key		**Responses by authors**
	Reviewer: Colin Mathers	
M1	P5.line 8. Is gastric cancer a commonly understood term for stomach cancer? I believe the latter term is more in line with cancer classification terminology, but may be have spent	Changed as suggested

	too long buried in disease coding discussions.	
M2	P6. Line 36. I think STEPS is the correct acronym, not STEP	Changed as suggested
M3	P7. Line 47. I am not an expert on dietary analysis, but standardization of intakes to 2000 kcal per day seems too low for many developed countries today. Is one global figure really appropriate?	The mean national energy intakes across the numerous dietary surveys we identified were actually in a fairly narrow range around 1850 to 2250 kcal/day. Intakes also varied by age, being generally higher among younger adults, and lower among older adults. The main purposes of energy-adjustment are to standardize the comparisons across different countries and subgroups (because most dietary guidelines are standardized to a 2000 kcal/d diet) and to reduce measurement error from diet surveys due to generalised over- or under-reporting of all foods within any subgroup.
M4	P8. Line 21. Little detail is given here of the Bayesian hierarchical model, although it is said that the authors developed it. However, the methods annex says that this model was DISMOD-III (without further reference or explanation). I assume this is the DISMOD tool used in GBD 2010 study, which I understood was being referred to as DISMOD-MR rather than DISMOD-III. There needs to be some more detail on the modeling methods and assumptions as well as a reference to documentation of DISMOD-MR. The authors should either clarify or reference the model assumptions and methods incorporated in DISMOD-III. They do mention that a cubic spline model was used for estimating age patterns, was a similar approach taken for modeling of time trends?	The provenance of the model has been clarified (and 'Dismod III' changed to 'DisMod-MR'). More details of the model and our imputation methods have been provided in the main text with further elaboration in the Methods Supplement. Change through time was modeled but outputs are reported for just 2 time points (1990 and 2010). This has been clarified.
M5	P48, line 24. This sentence seems to suggest that the standard deviations (of the mean effect sizes in the table above?) were estimated separately outside DISMOD using another regression model. This seem quite odd and deserves some more explanation, or rewording if I am misunderstanding it.	The misleading words 'Standard deviations were estimated using a regression model' have been deleted. The estimates (of Monte Carlo standard errors) were made within DisMod-MR
	Reviewer: Kenji Shibuya	
S1	Page 8, Figure 2. There seem to be 8 age and sex strata that follow their own very clear curve that might represent some kind of stratifying or confounding variable. If this variable could be identified and included in the regression model it would potentially significantly increase the R-squared statistic for this model. The authors should investigate these 8 strata and see if they arise from a specific study, region or period, and consider developing a slightly more detailed regression model based on this. It is possible that this would improve overall accuracy of the model through better development of the crosswalk regression.	These 8 data points are from specific study sites (2 of the 3 InterMap sites in China). Adding a marker variable for data from these sites increases the r^2 to 0.79. However this variable does not seem able to serve as an appropriate stratifying variable for data outside the cross-walk dataset (e.g. it does not correspond to a type of study design), and could also simply be due to random variation (at survey site level). We have not therefore included the extra variable. See revised text for the footnote to Figure 2. These outliers do have an influence on increasing the uncertainty in the upper part of the data range.
S2	Page 8, Bayesian hierarchical model There is no test of predictive validity in the present study. Thus, I cannot judge the performance of the present approach. If possible, it would be useful to see a sub-analysis using only countries and regions with large amounts of	We recognize and appreciate this challenge: the problem is that there is simply no true "gold standard" against which to validate results. All instruments include measurement error, whether 24-hour urine or self-reported diet, with potential for both random error and systematic error (bias). As one

	available data, perhaps presenting the results of some form of cross-validation process. For example, the countries with large amounts of available data could be divided into training and validation datasets, and the predictive accuracy of the model shown for this restricted data set.	example, 24-hour urine data tended to be subnational (less representative), whereas dietary data tended to be national but have larger measurement error. Our model takes advantage of all available raw data in the world; a cross-walk to render self-reported dietary values more comparable to 24-hr urine values, based on empirical relations between these measures; and then the relation between these data and global country-level covariates (GDP, age, sex, principal component analysis of food supplies) in a Bayesian hierarchical fashion. Outputs from the model are provided in the Supplementary Figures. For regions in which greater data was available the fits are fairly good. However, the key challenge is that a comparison of the raw vs. estimated results does not indicate or test validity. The raw data themselves are measured with error, which is variable across surveys, types of instruments, countries, and within subgroups within countries. Thus, when there may be any "mismatch" of the raw vs. estimated data, one cannot determine, in any formal fashion, whether the mismatch represents "error" in the imputation model or actually an improvement over the raw data. The assumption of our Bayesian approach is that the final estimated data, informed by both the raw data, covariates, and regional hierarchy, are closer to the 'truth' than any isolated datapoint. We have added this assumption to the Discussion; and a more detailed discussion of these issues to the appendix materials.
S3	Page 10, Results The authors did a great job providing relative uncertainties in Figure 3. However, as indicated in Table 1, the data sources are highly skewed to high- and middle- income countries where examination surveys are in place. There were no data available for over 120 countries. The current approach is a hybrid of empirical data, models and extrapolations. The major drawback of this approach is that the prediction for countries with limited data points tends to be derived from covariates or extrapolations from neighbouring countries, which is arbitrary and thus makes the assessment of estimation less compelling than that of, for example, child mortality series. Very few surveys are available in sub-Saharan African and South and Southeast Asian regions. As there is limited correlation between sodium and national income, I wonder how valid the current results, in particular on trends, would be in these regions where the next epidemic of stroke will take place. From the figure in the web appendix, it appears that for some of these regions with sparse data, the prior and the posterior distribution are very similar. This may indicate that the choice of prior is essentially driving the results for these regions. The authors should offer caveats in the text indicating the extent to which the results are driven by the priors.	In countries and regions with sparse data, key drivers of the estimated results included the FAO components, which are significantly related to the raw data in other countries. Raw data were available in most of the large nations in the world, including many low and income countries. But, we acknowledge the missing data in many countries – indeed, this missing data on intakes in many countries was the major underlying justification for the current investigation. We have added the following text to the discussion to deal with these important points: "For regions and their constituent countries where primary exposure data were limited or absent (e.g. Sub-saharan Africa, central and Latin America, Andean), relative uncertainty is correspondingly greater: their Monte Carlo standard errors exceed 9% their means, compared to 2.5% for the relatively data rich region of Western Europe.". .." For data deficient regions, final estimates correspond to their priors, which depend in turn on the predictive ability of covariates. Model outputs for such regions– Figures S1, S3, S4) show that the coefficients for the fixed effects of the FAO diet composition components (especially component 1) are larger than the coefficients on income (which were expected to be low). The missing raw data on sodium intakes in much of the world was a major motivation for undertaking the present investigation. Substantial reduction of the uncertainty in these estimates must await the carrying out of good quality national surveillance studies so that the dependence of final estimates on priors, and their associated uncertainties, is reduced."
S4	Supplemental material page 2, factor analysis More information is necessary about the factor analysis used to derive the food components of the Bayesian model. This factor analysis appears	This text has been changed to clarify that principal components analysis was used.

	to be very important for driving country-specific outcomes (see e.g. the fixed effects shown for the region- and sex-specific model outcomes in the following figure). However, the method for this factor analysis is not clear, the text and Table S1 seem to present different results, and the description appears to confuse factor analysis and principal components. The authors need to clarify this analysis by:	
S4.1	1. Indicating whether the loadings shown in Table S1 are from the eigenvectors of a principal component analysis or from a factor analysis	Text changed to clarify that they are from principal components analysis
S4.2	2. If the loadings in Table S1 are derived from a factor analysis the authors should describe the rotation method used in the text on page 2	N/A
S4.3	3. If not from a factor analysis, the authors should not refer to this section of their analysis as a "factor analysis" but as "dimension reduction through principal component analysis"	Done.
S4.4	4. If factor analysis was used, indicate whether they used a principal factor extraction method or a maximum likelihood method	N/A
S4.5	The authors also should present the eigenvalue of each factor, variance explained by each factor, and total variance explained by the four factors. The authors have used the Kaiser criterion (all factors with eigenvalues > 1) to select the number of factors to include. From looking at Table S1 and Table S2, however, it appears that the fourth factor may be unnecessary, and that even though its eigenvalue is greater than one it contributes very little to variance explained.	New table MS 2 (in the Methods Supplement) provides the requested values.
S4.6	The authors' interpretation of the meaning of each factor/principal component also seems disputable. For example, component 1 appears to be a meat-and-alcohol factor, while component 2 is a contrast between saturated fats and vegetables/fibres. The authors state that "factor 4 has the highest loading on sugars, stimulants and saturated fats" but in fact the highest loading for sugars and stimulants is in factor 1, and the highest loading for saturated fats in factor 2. It appears that the text on page 2 does not match the factors reported in Table S1, and the authors need to clarify this. These factors also look as if they might represent regional dietary structures: component 1 may represent a western diet and component 4 an east Asian diet, for example. If so, there may be significant collinearity between the factors and the regional random effects: the authors should check this possibility by comparing factor values between regions or nations. Given the importance of the factor analysis of food inputs to calculation of sodium levels, this information needs to be clarified and, if necessary, the model should be rerun with a revised factor analysis.	Text re-drafted: Factor characterisation changed in response to comment. We have revised the text of the Discussion to acknowledge importance of priors in data deficient regions (see response to S3 above).
	Reviewer: Thorhallur Ingi Halldorsson	
H1	It's quite confusing when you first state that	Estimates were produced for 1990 and 2010 but data from all

	sodium intake was estimated in adults in 1990 and 2010 (objectives) and then in the "Data sources and eligibility" you say that surveys conducted between 1980 and 2010 were used. This 1990 and 1980 issue needs further clarification in the abstract.	years was used to estimate changes over time. A sentence to this effect has been added to the methods on p 7. (Word limits prevent additions to the Abstract.)
H2	In the result section of the abstract you say that men had approx. 10% higher intake than women. Is that absolute value or is it accounted for by difference in weight (or energy intake). Men have absolute higher intake of sodium (an other nutrients) as men have higher body weight on average. If highlighted in the abstract I would say "As expected men had ~10% higher sodium …." (and throughout the manuscript).	We agree that sex differences in sodium intake reflect sex differences in food intake which are in turn determined by sex differences in energy expenditure. We have added 'as expected' on p 10.
H3	Please state in the abstract what the recommended sodium intake is in g/d so the mean intakes can be compared to that number in the abstract. Also report % of countries above this recommendations (as done later in this paper). This is the main result from this study and it needs to be highlighted in the abstract (not just mentioned in the conclusion in the abstract)	We have added the WHO recommended limit to the abstract to help readers calibrate our reported levels.
H4	Also highlight the % above recommendation in key messages	We have also added the WHO recommendation to the Key points.
H5	Methods, page 6, line 16. It might be appropriate to add a reference to a suitable introductory book (or chapter) where hierarchical Bayesian imputation is described.	We have added a reference in the methodology supplement to the special Lancet addition on GBD2010 – which in turn has further references on statistical methodology.
H6	Methods, page 7, line 11 and lines 51-59. You mention differences in accuracy when relaying on 24h urine vs. dietary assessment methods. Concerning the dietary assessment methods a very large variability in accuracy for estimating sodium is expected. As an example FFQs can rank individual subjects according to intake accurately but absolute values are poorly determined. On the other hand diet records and recalls should be much more accurate on a group level (although records may influence subjects eating habits). If a range of dietary methods were available from one (or comparable) countries differences in estimates between methods could be examined and reported (is FFQs are suitable for inclusions ?). Limitations of using estimates from different dietary methods should at least be mentioned and acknowledged.	We agree that diet assessment methods will likely vary in the validity of their estimates of sodium intake and extracted data was coded for diet assessment quality (coding of individual studies provided in the last column of the Supplementary Table). However this was dropped from the meta-regression when it failed to add to predictions. In addition, while 24-hour urine data have less random error, such data are limited by incomplete collections (which may be non-random) as well as, very often, lack of national representativeness. Thus, as described in responses above, no raw data can be considered a perfect gold standard. These issues, together with the missing data in many countries, were the major motivations for the present investigation. These issues have been added to the Discussion. Note that there is a major heterogeneity in dietary sources of sodium across regional food cultures. Some methods may work best where most sodium is added before foods reach the household, others where much is added in food preparation and at the table. The data are not available to tease these effects out.
H7	Methods, page 8, line. You mention here and in abstract non-urinary losses (sweat). Although only 10% loss can be expected on average (as you state) is it possible that variation between regions could be partly explained by non-urinary losses (when relaying on 24h urine). In summary is it possible that non-urinary losses are greater in Sub-Saharan Africa compared to Scandinavia (on average).	We agree and have noted in the text that the use of urine based measures unadjusted for sweat losses may introduce biases across regions. We did not believe that sufficient data were available to adjust or correct for this in a valid fashion.
H8	Results, page 10, line 1: Please state the mean number of subjects in each survey and range (for	It should have been made clear that this information is provided in the Supplementary Table (which the reviewer

	these 245 surveys), preferably for 24h urine and dietary surveys separately. Reporting the total number of subjects in all these 245 surveys might also be appropriate	seems to have missed). The main text has now been altered appropriately to make this clear.
H9	Result, page 10, line 18: could you explain in slightly more detail what a “data point” refers to in your study.	The meaning of ‘datapoint’ has now been spelt out.
H10	Result, page 10, line 44. Again I would add “as expected” when it comes to this gender difference	Done
H11	Result, page 11, line 6. What does “UI” stand for (has it been written out in full for the first time prior to this ?)	‘Uncertainty interval’ now spelt out
H12	Results page 12, lines 15-21: When you say that 99.2% or the world’s population exceeds the WHO recommendation does that refer to that the average in 99.2% of the countries exceeds the recommendation?? It would also be informative if you could report the % of subjects in each country that exceed the recommendations based on all these surveys (even though the mean is above the variation between countries may differ and it is informative to know the % of subjects above/or below the recommendation in each country).	Yes, the correct statement is that the national means in countries whose total adult population accounts for 99.2% of the world’s adult population exceed this recommendation. We have revised the text accordingly. To directly calculate the % of individuals within each country above or below the cutpoint would require individual-level data for each country. For many surveys, we had data on subgroup means by age and sex, not individual-level data. In addition, even for nations with individual-level data, as described in prior responses, these were most often dietary surveys (which include greater error) or subnational urine surveys. Thus, we do not believe these raw, individual-level data necessarily would provide more accurate results than subgroups means by age and sex that are informed by all available data including key covariates. We agree that this information would be useful if it could be robustly provided. However we consider that it is more robust to allocate the whole population of countries on the basis of their estimated mean intakes – as in the existing text.
H13	Discussion page 13, line 41: You say “Numerous other organizations have systematically reviewed the evidence and concluded that high intakes of sodium increase cardiovascular risk.11 27 30” Can you be more speck in terms of what “high” means here ?	Now changed to: ‘Numerous other organizations have systematically reviewed the evidence and concluded that high intakes of sodium (above 1200 to 2300 mg/d) increase cardiovascular risk.’
H14	Discussion, page 15, line 32. Using the word “optimal” here might be too strong.	This has been redrafted around the idea of a gold standard for sodium surveillance.

VERSION 2 – REVIEW

REVIEWER	Kenji Shibuya University of Tokyo, Japan I am one of the core members of the GBD 2010 study, but I am not involved in any aspect of this study.
REVIEW RETURNED	01-Nov-2013

THE STUDY	I've looked at the re-review below, and I'm satisfied with it. The authors have answered all my questions and made changes where necessary, and it's ready to accept.

REVIEWER	Thorhallur I Halldorsson

	Faculty of Food Science & Nutrition, University of Iceland
REVIEW RETURNED	10-Oct-2013

GENERAL COMMENTS	All my concerns have been dealt with and I have no additional concerns or comments