PEER REVIEW HISTORY

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This paper was submitted to a another journal from BMJ but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open. The paper was subsequently accepted for publication at BMJ Open.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Global, Regional, and National Consumption of Major Food Groups in 1990 and 2010: A Systematic Analysis Including 266 Country-Specific Nutrition Surveys Worldwide
AUTHORS	Micha, Renata; Khatibzadeh, Shahab; Shi, Peilin; Andrews, Kathryn; Engell, Rebecca; Mozaffarian, Dariush

VERSION 1 - REVIEW

REVIEWER	Marques-Vidal, Pedro Institute of Social and Preventive Medicine
REVIEW RETURNED	08-Dec-2014

GENERAL COMMENTS	Reviewer: Pedro Marques-VIdal The authors conducted a considerable work by analyzing a large number of local, regional and nationality representative studies on dietary intake. They complemented their analysis including data from the food and agricultural organization food balance sheets for all countries available. Overall, the manuscript is well written and provides important information regarding differences in dietary intake between countries, together with their trends. The statistical methods used have been applied in other studies and are beyond the review words knowledge, so a specific statistical reviewing might be necessary. Most supplementary graphs are provided in a limited number of pages, which makes them extremely small. Thus, their interpretation is almost impossible as the numbers in the graphs cannot be read by the majority of readers, even after printing the documents in an A3 format. Major comments 1. In the introduction, the authors indicate in the first paragraph that they are focusing on dietary prevention of cardio metabolic diseases. Still, in the next paragraphs, the authors expand this dietary prevention to other diseases such as cancer and diabetes. Thus, the authors introduce cancer and diabetes also in the first paragraph. 2. Page 56, lines 24 to 51, it is unclear why the authors used seafood from the data sources and PUFA from the FAO organization food balance sheets. The authors use the seafood and fish consumption from the FAO food balance sheets instead of the PUFA. This is also more in agreement with their status in the introduction, lines 20 to 27, where they state that "food-based guidance greatly facilitates public education " 3. In all 187 countries, women consumed more fruits and vegetables

	than men. This extremely homogeneous finding might be related to the fact that all dietary intakes were standardized for 2000 kcal per day. As women tend to consume less calories than men, it is thus possible that the higher consumption of fruits and vegetables in women may simply be due to the fact that both genders are supposed to consume a 2000 kcal diet. The same applies for unprocessed red meat and processed meat consumption, which is systematically higher in men. Although the reviewer agrees that the standardized “2000 kcal a day” simplifies comparisons between genders, still it might lead to the false conclusion that women eat systematically more fruits and vegetables in absolute amounts than men. Conversely, the 2000 kcal per day standardization decreases the difference in meat consumption between genders. The same comment applies to age groups, as caloric consumption of older subjects is usually lower than of younger subjects and does some differences might be either increased or decreased. The authors should provide in the discussion a word of caution against making such conclusions and remind the reader that the values provided in the tables suppose the same caloric consumption for both genders and for all age groups. 4. Changes in food consumption between 1990 and 2010. The statistically significant increase in fruit consumption in South Asia is mainly driven by the increase in India, which outweighs the decreases in the neighboring countries. On page 15, lines 15 to 17, the mean absolute decrease in whole-grain consumption in Congo (-372 g/day) corresponds to a decrease of over 1200 kcal per day, which is considerable. Do the authors have any information whether caloric consumption decreased so much in this country? Similarly, the large increases in Mauritius and Cape Verde could eventually impact obesity levels in these countries. Again, do the authors have any information regarding trends in obesity levels in Mauritius and Cape Verde which could confirm such large increases in whole-grain consumption? 5. Page 20, limitations paragraph. The authors should acknowledge that the food and agricultural organization food balance sheets for several countries, namely the sub-Saharan countries, rely mostly on estimations and might thus provide relatively “soft” data. The authors should also indicate that no data that regarding fat intake was modeled. Minor comments. 1. In the appendix the FAO database is indicated as “food and agriculture organization food disappearance balance sheets”, while page 9, lines 3 to 4 it is indicated as “food and agricultural organization availability data”, and some lines afterwards it is indicated as “food and agricultural organization food balance sheets”. The authors should use a single destination for these data to wealth to the manuscript 2. In page 9, lines 44 to 45, table 3 is mentioned before table 2. 3. Figure 5, the font in the graphs is extremely small and not easy to read. The reviewer hopes the graphs will be presented on a larger scale for future readers. 4. eTable 2: Page 59: lines 29 to 33 and 37 to 40 the studies in Iceland included only children aged less than 16. Thus, according to figure 1, they should not be used for data analysis. Page 62, lines 5 to 7: according to etable 1, Argentina should be in Southern Latin America and not in central Latin America. Page 62, lines 37 to 41: the NHANES studies are conducted almost every two years. The authors should indicate why they selected to use only the surveys between 2003 and 2008 while other data that was available at least till the nineties.

	5. eFigure 4. Please indicate what the vertical dotted lines mean. 6. The graphs in figure 7 are extremely small and even printing on an A3 format does not help much. The authors should indicate in the legend that these regressions were conducted using data from etable 2 and, as some studies only provided joint data for people aged 0 to 100 years, age groups 0 to 20 must be provided. The graphs with the fixed effects, random effects and median adjusted relative errors are so small that their interpretation is impossible.

REVIEWER	Cafiero, Carlo UN FAO, Statistics Division
REVIEW RETURNED	16-Mar-2015

GENERAL COMMENTS	The article presents estimates of national average consumption levels of key foods related to cardio-metabolic diseases by country, age and sex, in 1990 and 2010. Estimates are obtained from data of dietary intake surveys, combined with national food availability data from FAO Food Balance Sheet data for countries, years and foods not covered by surveys. Key results point to the very low number of countries where average consumption of fruits and vegetables would be sufficient to cover minimally recommended levels to reduce risk of cardio-metabolic diseases. If sufficiently reliable, results would indeed be important to inform policies and priorities for improving global health, by highlighting areas where food intake patterns are particularly unbalanced and possibly conducive to cardio-metabolic diseases. Given their expertise, these reviewers focused specifically on statistical aspects of the analysis presented, and in particular on the use of FAO Food Balance Sheet data. The conclusion of the review is that the evidence provided with the article is insufficient to determine whether the empirical evidence truly supports some of its key results. Unfortunately, no information is provided to allow us to evaluate if, and how, uncertainty associated with estimates of national per capita availability of individual foods derived from FBS has been considered in the analysis. Our understanding of the way in which FBS data has been used to complement the information obtained from dietary intake surveys suggests that a model has been defined to link average intake levels of key foods to national availability data and parameters have been estimated based on data from countries and years where both sources of data are available. Estimated parameters are then used to inform the prediction of food intake levels in years and countries where only FBS data are available, in a typical out-of-sample projection. Reliability of such model based prediction crucially stand on two assumptions: a) that there is no systematic difference in the way in which food intake is linked to food availability between the two groups of countries and that, b) errors in the FBS food availability data across countries are independent from their level and identically distributed in the same two groups of countries. The authors should discuss the extent to which the two assumptions on which are met for the specific lists of countries and years included in the analysis. They may want to explicitly consider the fact that, while FBS can be taken as sufficiently reliable assessments of the overall availability of food (expressed by its dietary energy content) for most countries, uncertainty (and potential bias) in estimates of the quantity available of individual foods is quite

	high for many countries, and suggest ways in which the Hierarchical Bayesian model they use takes that into account. Our fear is that systematic problems with the levels of specific foods reported in FBS due to factors not controlled for in the model could cause problems with the results. Failure to take into account the fact that, for example, availability of fruits or vegetables may be significantly under reported in countries where fruit and vegetable consumption is mostly obtained from sources that elude national statistics, might contribute to the insufficient levels of fruits and vegetable consumption predicted in those countries, especially if these are the typical countries for which no dietary intake survey are available. Our suggestion to the editor is to encourage re-submission after a revision that would include the above aspects. There are a number of other specific points that could be taken in the consideration in view of a revision. • The data are separated into two time points (1990 and 2010) and then inputted to a Bayesian Hierarchical model. To do this, surveys carried out between 1980 to 1997 were used to inform the 1990 period and surveys cared out between 1997 and 2010 were used to inform the 2010 period. There should be a way to use time as a continuous variable and eliminate this problem with classification. • Consumption levels seem to have been adjusted for energy levels before use in the model and analysis. Nutrient intake is standardized to a 2000 kcal/day diet. However, this means that all of the units should be properly expressed as g/2000 kcal, rather than g/day. • To of page 10: Statement is currently written as: “High vegetable intake was primarily as legumes in Southern Sub-Saharan Africa and Tropical Latin America”. Statement should read that “High vegetable intake was primarily in terms of / due to high consumption of …” • Bottom of page 16: “No regions or countries had statistically certain increases or decreases in processed meat intake between 1990 and 2010.” Statement should read that “No regions or countries had statistically significant …” • Top of page 18: “Overall, diet is now the single leading preventable cause of noncommunicable diseases.” Should read that “unhealthy diet is now the single leading…” • Top of page 20: “Intakes were adjusted for total energy, reducing measurement error; and sensitivity analyses without energy-adjustment were similar.” The performed adjustment is a data transformation and should not reduce measurement error.

VERSION 1 – AUTHOR RESPONSE

Comments from Reviewer #1 (Prof. Pedro Marques-Vidal, Lausanne University Hospital (CHUV)):

The authors conducted a considerable work by analyzing a large number of local, regional and nationality representative studies on dietary intake. They complemented their analysis including data from the food and agricultural organization food balance sheets for all countries available. Overall, the manuscript is well written and provides important information regarding differences in dietary intake between countries, together with their trends. The statistical methods used have been applied in other studies and are beyond the review words knowledge, so a specific statistical reviewing might be

necessary. Most supplementary graphs are provided in a limited number of pages, which makes them extremely small. Thus, their interpretation is almost impossible as the numbers in the graphs cannot be read by the majority of readers, even after printing the documents in an A3 format.
Response: Thank you – In response to this comment, we have enlarged all supplementary Figures 1 through 7 (revised Data Supplements).

Major comments

1. *In the introduction, the authors indicate in the first paragraph that they are focusing on dietary prevention of cardio metabolic diseases. Still, in the next paragraphs, the authors expand this dietary prevention to other diseases such as cancer and diabetes. Thus, the authors introduce cancer and diabetes also in the first paragraph.*
Response: Agreed, suboptimal diet is the single leading preventable cause of non-communicable diseases (including cardiovascular diseases, cancer and diabetes mellitus), making improving diet a public health priority. We have clarified this important point in the Abstract (page 2), "Article Summary" (page 3), Introduction (page 4), and Table 2 title.

2. *Page 56, lines 24 to 51, it is unclear why the authors used seafood from the data sources and PUFA from the FAO organization food balance sheets. The authors use the seafood and fish consumption from the FAO food balance sheets instead of the PUFA. This is also more in agreement with their status in the introduction, lines 20 to 27, where they state that "food-based guidance greatly facilitates public education".*
Response: Based on our separate assessment of impact on coronary heart disease risk, our primary metric was n-3 PUFA. Therefore, we focused on that FAO variable, which for consistency was used for both n-3 PUFA and seafood. In fact, FAO seafood and n-3 PUFA variables are highly intercorrelated (national Spearman r=0.91). You are correct it would have been better to use the seafood variable, and we will make this modification in future work. For the present investigation, these imputations were completed as part of a multi-year, multi-investigators process, which would be unrealistic to going back and revise retroactively. We have added to the limitations (page 19) that "Several combinations of foods in the FAO food balance sheets were possible, and future investigations can further expand upon potential combinations."

3. *In all 187 countries, women consumed more fruits and vegetables than men. This extremely homogeneous finding might be related to the fact that all dietary intakes were standardized for 2000 kcal per day. As women tend to consume less calories than men, it is thus possible that the higher consumption of fruits and vegetables in women may simply be due to the fact that both genders are supposed to consume a 2000 kcal diet. The same applies for unprocessed red meat and processed meat consumption, which is systematically higher in men. Although the reviewer agrees that the standardized "2000 kcal a day" simplifies comparisons between genders, still it might lead to the false conclusion that women eat systematically more fruits and vegetables in absolute amounts than men. Conversely, the 2000 kcal per day standardization decreases the difference in meat consumption*

between genders. The same comment applies to age groups, as caloric consumption of older subjects is usually lower than of younger subjects and does some differences might be either increased or decreased. The authors should provide in the discussion a word of caution against making such conclusions and remind the reader that the values provided in the tables suppose the same caloric consumption for both genders and for all age groups.

Response: We fully agree with the Reviewer, and we have now clarified in the Discussion that our findings represent compositional, not absolute intakes. As this Reviewer astutely raises, the ultimate aim is understanding the health implications of diet, for which a key question is: what is the most relevant dietary metric for health: absolute intake, independent of calories, or relative intake, as a proportion of calories? Total energy intake varies among individuals due to age, body size, lean body mass, physical activity, and so forth. For example, if a large active man consumes 3000 kcal/d including one apple, and a small women consumes 1500 kcal/d with the same apple, do they receive similar benefit? Or, does the man need to consume 2 apples to receive the same benefit due to larger size, muscle mass, and physical activity? Although this question is not easily addressed, associations of diet with disease are generally stronger following energy-adjustment, which may relate both to biologic relevance of composition (energy-adjusted intake) and also reduced measurement error with energy adjustment. Thus, in a sense, women do systematically consume "more" fruits and vegetables. Yet, we fully agree with the reviewer that we should be clear, and we have clarified in the Discussion (page 18) that reported intakes represent compositional, not absolute intakes.

4. *Changes in food consumption between 1990 and 2010.*

(a) *The statistically significant increase in fruit consumption in South Asia is mainly driven by the increase in India, which outweighs the decreases in the neighboring countries.*

Response: This is a good point - among South Asian countries, the only statistically significant increase in fruit consumption occurred in India; in all other South Asian countries intakes increased nonsignificantly, with the exception of Afghanistan where intakes decreased nonsignificantly. We have clarified this in the Results text (Changes in Consumption between 1990 and 2010, page 14).

(b) *On page 15, lines 15 to 17, the mean absolute decrease in whole-grain consumption in Congo (-372 g/day) corresponds to a decrease of over 1200 kcal per day, which is considerable. Do the authors have any information whether caloric consumption decreased so much in this country? Similarly, the large increases in Mauritius and Cape Verde could eventually impact obesity levels in these countries. Again, do the authors have any information regarding trends in obesity levels in Mauritius and Cape Verde which could confirm such large increases in whole-grain consumption?*

Response: This is an interesting observation. Recent work by others (www1.imperial.ac.uk/publichealth/departments/ebs/projects/eresh/majidezzati/healthmetrics/metaboliciskfactors/) has focused on characterizing global trends in body-mass index (BMI) since 1980 (Finicane MM et all Lancet 2011). Between 1980 and 2008, mean BMI worldwide increased by 0.4-0.5 kg/m^2 per decade. Although mean BMI levels were generally higher in Mauritius, followed by Cape Verde and Congo, similar increasing trends over time were seen, except for men in Congo for whom

BMI appeared relatively unchanged.

Such ecologic comparisons between population diet changes and outcomes can provide possible hints, but are fraught with danger due to ecologic confounding, i.e. multiple other differences and unrelated trends over time between nations. Consequently, the Global Burden of Diseases Study and similar modeling exercises perform comparative risk assessment analysis to assess potential impacts on disease burden, utilizing separate information on causal effects of dietary factors from controlled trials and prospective cohorts to understand the impact on health. In other words, the increases in whole grains in Mauritius and Cape Verde could certainly be reducing obesity. But, if the counterfactual (the obesity trend in the absence of such whole grain change) is a very large obesity rise due to other, unrelated factors, then these countries could easily still be experiencing obesity rise, it's just that the whole grain consumption is reducing this rise.

5. *Page 20, limitations paragraph. The authors should acknowledge that the food and agricultural organization food balance sheets for several countries, namely the sub-Saharan countries, rely mostly on estimations and might thus provide relatively "soft" data.*

Response: This is an excellent point. We have clarified in the Discussion (limitations, page 19) that FAO data have several inherent limitations, and that such data often overestimate individual-based intakes (Del Gobbo LC et al Am J Clin Nutr 2015). This is a major strength of our investigation – to use these data in a multi-level model which allows them to provide additional information across all countries and years, but also be appropriately adjusted to account for their error and variation based on relationships with multiple individual-dietary surveys in countries having both.

Minor comments.

1. *In the appendix the FAO database is indicated as "food and agriculture organization food disappearance balance sheets", while page 9, lines 3 to 4 it is indicated as "food and agricultural organization availability data", and some lines afterwards it is indicated as "food and agricultural organization food balance sheets". The authors should use a single destination for these data to wealth to the manuscript.*

Response: Thank you – We have consistently indicated these data as "FAO food balance sheets" throughout the Manuscript and Data Supplements.

2. *In page 9, lines 44 to 45, table 3 is mentioned before table 2.*

Response: In response to this comment, the order of Table 2 (currently Table 3) and Table 3 (currently Table 2) is reversed in the revised Manuscript.

3. *Figure 5, the font in the graphs is extremely small and not easy to read. The reviewer hopes the graphs will be presented on a larger scale for future readers.*

Response: Each panel of Figure 5 is currently presented on a larger scale.

4. *eTable 2.*

(a) *Page 59: lines 29 to 33 and 37 to 40 the studies in Iceland included only children aged less than 16. Thus, according to figure 1, they should not be used for data analysis.*

Response: This is a good point. eTable 2 had presented all surveys identified in adults and children (in addition to Iceland, surveys in children were also presented for Sweden and the UK). We have removed these surveys in children only, which were not used in the analysis, in the revised eTable 2.

(b) *Page 62, lines 5 to 7: according to etable 1, Argentina should be in Southern Latin America and not in central Latin America.*

Response: Correct – In the revised eTable 2 the survey in Argentina is now listed under Southern Latin America.

(c) *Page 62, lines 37 to 41: the NHANES studies are conducted almost every two years. The authors should indicate why they selected to use only the surveys between 2003 and 2008 while other data that was available at least till the nineties.*

Response: At the time these datasets were collated, we decided to focus on studies having at least two 24-hr recalls whenever possible, to allow better estimation of within vs. between person variation. Thus, we excluded earlier cycles with only a single recall. The 2009-2010 data became available (first published in September 2011) during our cleaning and modeling, but after we had completed our systematic searches. Because our searches were systematic, we felt it inappropriate to add a new survey ad hoc, without repeating our systematic searches. In future work, we plan to use all available NHANES cycles. We have clarified this in the eTable 2 footnote.

5. *eFigure 4. Please indicate what the vertical dotted lines mean.*

Response: The vertical dotted lines correspond to the optimal consumption levels of each food (as presented in Table 2), in order to place the observed consumption levels in context. The requested clarification has been added to the eFigure 4 legend (revised Data Supplements).

6. *The graphs in figure 7 are extremely small and even printing on an A3 format does not help much. The authors should indicate in the legend that these regressions were conducted using data from etable 2 and, as some studies only provided joint data for people aged 0 to 100 years, age groups 0 to 20 must be provided. The graphs with the fixed effects, random effects and median adjusted relative errors are so small that their interpretation is impossible.*

Response: In response to this and previous related comment we have enlarged all supplementary Figures 1 through 7 (revised Data Supplements). We would like to clarify, and as further currently stated in eTable 2 footnote, that age range refers to age groups as reported; only data in individuals aged 20+ years were used to estimate worldwide intakes. However, for the model fits the aim was to test the fit of the model across all ages, and thus data on all available ages were included. We have clarified this important point in the eFigure 7 legend.

Comments from Reviewer #2 (Carlo Cafiero, Senior Statistician and Nathan Wanner, Junior Statistician, UN FAO Statistics Division):

1. *No information is provided to allow us to evaluate if, and how, uncertainty associated with estimates of national per capita availability of individual foods derived from FBS has been considered in the analysis.*

Response: The hierarchical Bayesian model accounts for the uncertainties in the relationship between the FAO food balance sheet data and the individual-level surveys and the other survey-level and country-level covariates in the model. In a sense, the uncertainty in these relationships partly incorporates the challenges of the food balance sheet data. Otherwise, formal statistical uncertainty of the food balance sheet data is not provided by FAO. We have added these important points to the model description (eAppendix, Data Supplements).

2. *Reliability of such model based prediction crucially stand on two assumptions: a) that there is no systematic difference in the way in which food intake is linked to food availability between the two groups of countries and that, b) errors in the FBS food availability data across countries are independent from their level and identically distributed in the same two groups of countries. The authors should discuss the extent to which the two assumptions on which are met for the specific lists of countries and years included in the analysis.*

Response: These are helpful comments. We fully agree that, for countries having zero survey level data, the performance of the model is dependent on the extent to which these assumptions are met. Undoubtedly, some systematic differences must exist, and errors in FBS are not identically distributed in all groups. We have formally assessed potential for interaction in the relationships between the individual survey data and FBS data in recently published work (Del Gobbo LC et al Am J Clin Nutr 2015). Statistical power for the fineness (strata size) of these evaluations is dependent on the numbers of surveys; and we also are obliged to consider the possibility of chance interactions with so many comparisons across regions and food groups. In that work, we identified that FBS data often overestimate individual-level intake data, and that the observed over- or under-estimation varied by world region for all food groups assessed. In the present work, the hierarchical imputation accounts for regional relationships prior to accounting for global relationships. Thus, the final estimates at least partly account for such heterogeneity. Ultimately, we fully agree that imperfections of the FBS data make them imperfect for assessing individual-level dietary intakes, which is the motivation for all these efforts. The present results represent the best available estimates of current global dietary intakes, which have limitations (Discussion, pages 19-20), but are far superior to crude national FBS data.

3. They may want to explicitly consider the fact that, while FBS can be taken as sufficiently reliable assessments of the overall availability of food (expressed by its dietary energy content) for most countries, uncertainty (and potential bias) in estimates of the quantity available of individual foods is quite high for many countries, and suggest ways in which the Hierarchical Bayesian model

they use takes that into account.

Response: These are excellent points. We have carefully considered these issues in a recent publication on this matter (Del Gobbo LC et al Am J Clin Nutr 2015). The lessons learned in this work are incorporated into our modeling for the present analyses (Methods, Discussion, eAppendix).

There are a number of other specific points that could be taken in the consideration in view of a revision.

1. *The data are separated into two time points (1990 and 2010) and then inputted to a Bayesian Hierarchical model. To do this, surveys carried out between 1980 to 1997 were used to inform the 1990 period and surveys cared out between 1997 and 2010 were used to inform the 2010 period. There should be a way to use time as a continuous variable and eliminate this problem with classification.*

Response: The model would have been too sparse with discrete years, so the choices were a binary time classification (as we used) or a continuous time classification. Based on review of some countries with multiple surveys, we felt it unlikely that continuous relationships were present for many dietary risk factors; or that, if present, we could accurately estimate the continuous trend with available data. Thus, we felt the binary stratification was more conservative to describe a broad overall trend between 1990 and 2010.

2. *Consumption levels seem to have been adjusted for energy levels before use in the model and analysis. Nutrient intake is standardized to a 2000 kcal/day diet. However, this means that all of the units should be properly expressed as g/2000 kcal, rather than g/day.*

Response: Yes, food consumption was standardized and evaluated as energy-adjusted g/d to 2,000 kcal/d, using the residual method (Data Retrieval and Standardization, page 7). Reporting intakes per 2000 kcal would imply standardization using the nutrient density method, which we did not use. It is most common in major nutrition reports that, following residual energy adjustment, results are reported as per day, with appropriate notation that these are energy-adjusted. We have followed this standard convention. In response to this and previous related comment from Reviewer #1 we have further clarified in the Discussion (pages 18-19) the distinction between energy-adjusted and absolute intakes.

3. *Top of page 10: Statement is currently written as: "High vegetable intake was primarily as legumes in Southern Sub-Saharan Africa and Tropical Latin America". Statement should read that "High vegetable intake was primarily in terms of / due to high consumption of …"*

Response: The relevant sentence has been revised as suggested (Results, page 9).

4. *Bottom of page 16: "No regions or countries had statistically certain increases or decreases in processed meat intake between 1990 and 2010." Statement should read that "No regions or*

countries had statistically significant …"

Response: Relevant statement has been appropriately revised (Results, page 15).

5. *Top of page 18: "Overall, diet is now the single leading preventable cause of noncommunicable diseases." Should read that "unhealthy diet is now the single leading…"*

Response: Thank you. The relevant sentence now reads "Overall, suboptimal diet is now the single leading preventable cause of non-communicable diseases, making food-based research a top priority for public health" (Discussion, page 16).

6. *Top of page 20: "Intakes were adjusted for total energy, reducing measurement error; and sensitivity analyses without energy-adjustment were similar." The performed adjustment is a data transformation and should not reduce measurement error.*

Response: Due to correlated errors between reported total energy and groups of foods/nutrients (systematic within-individual under- and over-reporting), energy-adjustment reduces measurement error (Willett WC, Nutritional Epidemiology, Oxford University Press, 2013). This is a strength of energy-adjustment, in addition to adjusting for differences in body size, physical activity, and metabolic efficiency (Discussion, page 19).

VERSION 2 – REVIEW

REVIEWER	Marques-Vidal, Pedro Institute of Social and Preventive Medicine (IUMSP), University Hospital of Lausanne
REVIEW RETURNED	21-Jun-2015

GENERAL COMMENTS	The authors conducted an extensive analysis of available dietary data from 113 countries. The statistical analysis is too complex for the reviewer to comment on, and expert advice should be asked for. The results are very comprehensive and are adequately presented. Issues 1. It would be easier for the reader that amounts be provided using international units (i.e. grams) instead of grams and ounces. At least, when amounts are provided in oz, the corresponding values in grams should be provided, so that the reader does not have to use a calculator. For example, in table 2, the amounts of fruits and vegetables are provided in grams whereas the amounts for nuts, seeds and whole grains are provided in ounces. This issue is further complicated for red meat, where USDG recommendations indicate 26 oz (approximately 740 g) but the optimal population intake is 100 g/week, making comparisons difficult. 2. Some optimal population intakes differ considerably from national dietary recommendations. As this paper might spark considerable media interest, a cautionary statement indicating that optimal population intakes might not forcibly

	be the ones recommended in selected populations should be provided. Namely, the very low levels of red meat intake might lead to the false inference that vegetarian (on-meat) diets are the optimal population diets. The importance of meat as a valuable source of protein and iron should be emphasized. 3. Similarly, the optimal fish intake of 3.5 servings/week is higher than most national dietary recommendations and although the reviewer acknowledges that it might be optimal regarding health, it might not be optimal regarding ecological impact, even if fish farming is increased. For instance, in Switzerland, 90% of the fish consumed is imported and fish farming capacity is reduced; hence, increasing recommendations from 2 to 3.5 servings/week would lead to a considerable increase in fish imports and not in fish farming. As another example, increasing vegetable consumption in China (one of the countries with the highest consumption) from 305 to the optimal value of 400 g/day means that it will be necessary to produce an extra 95 * 365 = 34.7 kg of vegetables per year considering that only one billion Chinese are adults (data.worldbank.org/indicator/SP.POP.TOTL) this amounts to 34.7 extra **million tons** of vegetables, not taking into account production and processing losses, which can be as high as 40%. This would need at least a 6% increment in China's vegetable production (www.geohive.com/charts/ag_vegetables.aspx). Overall, the reviewer believes that a statement indicating that optimal population intakes might not be achievable for all foods and everywhere due to ecological, production, cost or behavioral factors should be provided.

VERSION 2 – AUTHOR RESPONSE

Comments from Reviewer #1 (Prof. Pedro Marques-Vidal, Lausanne University Hospital (CHUV)):

Minor comments:
1. It would be easier for the reader that amounts be provided using international units (i.e. grams) instead of grams and ounces. At least, when amounts are provided in oz, the corresponding values in grams should be provided, so that the reader does not have to use a calculator. For example, in table 2, the amounts of fruits and vegetables are provided in grams whereas the amounts for nuts, seeds and whole grains are provided in ounces. This issue is further complicated for red meat, where USDG recommendations indicate 26 oz (approximately 740 g) but the optimal population intake is 100 g/week, making comparisons difficult.
Response: Thank you – In response to this comment we have provided the corresponding values in grams when amounts were provided in oz or servings throughout the manuscript (mainly Abstract, Results, Table 2). We would further like to clarify that the USDG recommendation includes all types of meats (red and processed meats, poultry) and eggs, and is thus not directly comparable to the 100 g/wk recommendation for red meat alone (Table 2).

2. Some optimal population intakes differ considerably from national dietary recommendations. As this paper might spark considerable media interest, a cautionary statement indicating that optimal population intakes might not forcibly be the ones recommended in selected populations. Namely, the

very low levels of red meat might lead to the false inference that vegetarian (on-meat) diets are the optimal population diets. The importance of meat as a valuable source of protein and iron should be emphasized.
Response: Agreed – We have clarified in the Discussion (Strengths, page 19) that optimal consumption levels helped to place the observed consumption levels in context and allowed consideration of potential impacts on disease in a consistent and comparable manner across countries, but that these levels may differ in certain populations. We have also clarified that meat is a source of calories, iron, zinc, and protein particularly in the poorest populations of the world (Discussion, page 18).

3. Similarly, the optimal fish intake of 3.5 servings/week is higher than most national dietary recommendations and although the reviewer acknowledges that it might be optimal regarding health, it might not be optimal regarding ecological impact, even if fish farming is increased. For instance, in Switzerland, 90% of the fish consumed is imported and fish farming capacity is reduced; hence, increasing recommendations from 2 to 3.5 servings/week would lead to a considerable increase in fish imports and not in fish farming. As another example, increasing vegetable consumption in China (one of the countries with the highest consumption) from 305 to the optimal value of 400 g/day means that it will be necessary to produce an extra 95 * 365 = 34.7 kg of vegetables per year considering that only one billion Chinese are adults (data.worldbank.org/indicator/SP.POP.TOTL) this amounts to 34.7 extra million tons of vegetables, not taking into account production and processing losses, which can be as high as 40%. This would need at least a 6% increment in China's vegetable production (www.geohive.com/charts/ag_vegetables.aspx). Overall, the reviewer believes that a statement indicating that optimal population intakes might not be achievable for all foods and everywhere due to ecological, production, cost or behavioral factors should be provided.
Response: The global dietary data presented in the current work inform policies and priorities for improving global health. The optimal fish intake of 3.5 servings/wk was based on the mean observed consumption associated with lower disease risk in meta-analysis of clinical endpoints (2-4 servings/wk for CHD [Zheng J et al Public Health Nutrition 2012] and 5 servings/wk for stroke [He K et al Stroke 2004]). To enable comparison with major dietary guidelines we further presented global characteristics for the recommended target of 2 servings/wk (Results text, Table 3). We completely agree with the Reviewer that national priorities and policies for reducing diet-related illness should consider ways to address cost (Discussion, page 17), which is a major barrier to consumption particularly in the poorest populations, production, distribution, sustainability, local cultures and food practices, potential industry opposition, equity assurance, and political feasibility. We have added this important point to the Conclusions and Policy Implications (page 20).