Author’s response to reviews

Title: Gender inequalities in diet quality and their socioeconomic patterning in a nutrition transition context in the Middle East and North Africa: a cross sectional study in Tunisia

Authors:

Mohamed Abassi (mehdi-abassi@hotmail.fr)
Sonia Sassi (soniasassi@hotmail.fr)
Jalila El Ati (jalila.elati@rns.tn)
Houda Ben Gharbia (houda_bg_782002@yahoo.fr)
Francis Delpeuch (francis.delpeuch@ird.fr)
Pierre Traissac (pierre.traissac@ird.fr)

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Author’s response to reviews:

Authors’ response to reviewer reports

Reviewer #1

General comment: the manuscript discusses an important topic which investigates gender inequalities in diet quality. I believe the information is relevant to inform decision and practice. My specific comments are described below.

Authors’ response

We thank the reviewer for the detailed assessment of the paper and the overall positive comments.

Title: the setting is stated as northern Africa and middle eastern region. But in the methods section, it is indicated that the study was conducted in Tunisia. How does this go along?

Authors’ response
We thank the reviewer for the relevant remark. As we stated in the study area section, Tunisia is a country emblematic of the Middle East and North Africa (MENA) undergoing advanced nutrition transition situation featuring e.g. high prevalence of overweight/obesity and as marked women vs. men inequality detrimental to women typical of the MENA region. The countries of this region have in common deep socio-cultural roots (partly linked to the Muslim culture) though with between countries variation. So that our urban setting in Tunisia can be considered as a case study of such contexts featuring socio-cultural similarities in the MENA region, particularly North Africa.

To be more specific we added the setting area in the title of the revised version of the manuscript which is now “Gender inequalities in diet quality and their socioeconomic patterning in a nutrition transition context in Middle East and North Africa: a cross-sectional study in Tunisia”.

Introduction is well written by describing important information about the issue researched

The measurement is also detail and well explained

Authors’ response

Thank you for the positive comment.

* Methods/Measurement: have the authors considered factors which affect nutrient adequacy like bioavailability or presence of infections/enteropathy so that they can ultimately assume the consumed diet can provide the required amount of nutrients?

Authors’ response

We completely agree with the reviewer that a person’s disease state (e.g. infection, inflammation or enteropathy) may result in increased energy requirements, reduced energy intake or even increased nutritional losses. So that could impact the interpretation the measured dietary intake. Nevertheless, our study area is characteristic of an advanced epidemiological transition situation (also in a mostly urban area) where infections/enteropathies are residual. Also to bias gender inequalities estimates, this would require that women vs. men be differentially impacted by this pathologies and there is no evidence that this should be so. As for the bioavailability issue, we have discussed it somewhat, especially regarding gender inequalities pertaining to iron intake as % of requirements.
In the revised version of the manuscript, as suggested by the reviewer, we have discussed theses issue (cf the “strengths and limitations” paragraph at the end of the discussion).

* Analysis : the authors have also indicated that they have used a multi staged cluster sampling method? How did they account this in the analysis of the data?

Authors’ response

As stated in the methodology section the clustering was taken into account for all analyses using the svy (SurVeY) prefix in the Stata statistical analysis software: this function is specifically dedicated to the analysis of data collected from samples using complex sampling plans. It is applicable to most Stata analysis commands to take into account: - the weighting resulting from unequal probabilities of selection and/or post-stratification weights, - the stratification, - the correlations between errors terms due to the clustering.

We have tried to make it clearer in the revised version of the manuscript (cf methods section).

Line number 180: change the term "multivariate" in to "multivariable"

Authors’ response

This has been changed.

In most nutritional studies, desirability bias affects reporting of consumed food (either by over reporting of amount or variety or under reporting). How did you address this issue in this research in?

Authors’ response

We agree with the reviewer that desirability can be a source of bias in assessment of dietary intake by declarative methods. As underlined by the reviewer, it may affect the reporting of both the amount and the type of food.

As matter of fact, we acknowledged that issue in several instances either directly or indirectly :- in the methods section, we use a range of implausible energy intakes to sort out over or under responders, - in the beginning of the discussion with regards to energy intake, we discuss the issue that obese vs. not obese subjects could differentially underreport the amount of food they
consume, - also in the strengths and limitations paragraph of the discussion we directly acknowledge that desirability issue as a possible source of bias.

In the revised version of the manuscript, we have underlined that issue some more, especially when discussing why women would declare consuming more certain foods and men some others.

The conclusion is well driven from the results presented

Authors’ response

We thank the reviewer for the positive assessment.

Reviewer #2

This is an interesting and important paper that highlights the differences in food consumption patterns between males and females in Tunisia as the culture transitions from a traditional diet to a globalized diet. The careful teaching and tracking of dietary intake was particularly valuable in providing information that could be utilized in informing nutritional interventions in this population.

Authors’ response

We thank the reviewer for the detailed assessment of the paper and the overall positive comments.

Below are my suggestions to improve your manuscript:

1. I strongly suggest having the paper copy-edited by a native English speaker. There are several confusing sentences and the manuscript generally lacks concise language which, can lead to confusion in interpreting important points and can take up unnecessary space in your manuscript.
Authors’ response

The revised version of the manuscript has been extensively edited by a native English speaker, specialized in editing scientific manuscripts (so that we did not highlight all the related changes)

2. The most interesting points of this article relate to the cultural transition between traditional diets and diets based on imported market foods. However, this manuscript contains insufficient information on the traditional diet and the process of this transition for readers to understand the impact of diet transition on the people of Tunisia. Please provide more information on: a) a brief description of the makeup of the traditional diet, b) an estimate (if available) on the proportion of traditional food vs. foreign food in modern diets, c) any information on gender differences in food consumption with traditional diets, and d) a description of the comparison of the nutritional quality between traditional and globalized diets.

Authors’ response

We agree with the reviewer that the changes in dietary intake linked to the nutrition transition are important to document and understand, but maybe only as context information as from our opinion it is not the purpose of the study to document in detail the evolution from the “traditional” to the “modern” diet in the context.

Information on gender differences in consumption of the different food items (whether “traditional” or “modern”) is de facto presented in our figure 1 (which presents detailed consumption of 20 food groups) and the results paragraph (which also details some more the food groups). There are also several references on the more “modern” or “traditional” character of some of the food groups in the discussion section of the paper.

In line with the reviewer’s suggestion, in the introduction, we have added summarized information regarding the general characteristics of the shifts in food consumption in the context of the nutrition transition.

Also, in accordance with the reviewer’s suggestion, at several instances, we have supplemented the discussion with some more comments on “traditional” vs. “modern” foods (either regarding overall or women vs. men dietary intake).

3. You have alluded to differences in Tunisian gender social norms influencing food choices. This is a highlight of your paper, but more information is needed. Please briefly specify some of these social norms.
Authors’ response

We agree with the reviewer that the “social norms” in the context and most importantly the “unequal gender roles” which derive from these norms, are central to understand gender inequalities in the context of the nutrition transition. It is to be also underlined that in several instances in the initially submitted version of the paper, if we did not use the “social norm” explicit wording outside of the introduction, we indeed discussed how “unequal gender roles” (which are both part and a consequence of these norms) or gender “stereotypes” or “cultural preferences” could impact (or not) food consumption (food choices or even possibly within-household food allocation even if we did not found any evidence of the latter).

In accordance with the reviewer’s suggestion, in the revised version of the introduction we have added a sentence to give a few examples of how these social norms may impact gender roles.

4. Throughout the paper, please change the wording of all the references to “women vs. men [differences, inequities, etc.”] to “gender [differences, inequities, etc.”] This will increase the readability and word economy of the paper.

Authors’ response

We agree with the reviewer’s suggestion regarding readability. We have made the changes suggested by the reviewer as much as possible. We also added a point in the methodology regarding this use of the wording “gender” somewhat beyond its meaning in the context of social epidemiology.

5. On page 4, line 100, please briefly specify why this age range was chosen.

Authors’ response

This age range was originally chosen as the main aim of the “Obe-Maghreb” project (in which framework the dietary data was collected) was to study the obesity-anemia double burden among women of childbearing age.

According to the reviewer’s suggestion we have added this information in the methods section (subjects).

6. There are several places throughout the paper in which a P-value is presented without the related statistic. This is confusing. For all odds ratios, it is standard to present the OR values and
CI ranges rather than percentages. Likewise, if there were t-tests or other analyses done (for example in the sample characteristics section), please report those values.

Authors’ response

We have added in the footnote of table 1 (and also Figure 2) that comparison of % was performed using chi-square tests (taking into account sampling design). The other statistics are defined in the methodology section.

7. It is not necessary to specify where detailed data is not shown.

Authors’ response

This has been changed.

8. In the methods and results sections, please change the term "inequalities" to "differences," as the term inequality has negative connotations to it that are more appropriately addressed in the discussion.

Authors’ response

This has been changed.

9. Please briefly describe why the DQI-I was chosen over other available tools. If there is data on the reliability and validity for the DQI-I from your study or others, please describe this in the methods section. Please briefly describe the reasoning for the cutoffs.

Author’s response

Food consumption data is highly dimensional (many foods and nutrients) so that some sort of dimension reduction is necessary to understand dietary patterns, e.g. by the use of summary diet quality scores. The DQI-I is a composite index accounting for the overall quality of the diet by incorporating both nutrient and food group intakes which was adapted from the original DQI by the team of B. Popkin with the aim of “assessing of diet quality across diverse countries at different stages of the nutrition transition”. Beyond assessing only overall diet quality using a
single score/dimension, the DQI-I also enabled us to assess four different dimensions of the diet (variety, adequacy, moderation and overall balance). This would seem relevant in our nutrition transition context as it has been shown this transition can result in contrasted results regarding the different dimensions of dietary intake (e.g. the meta-analysis of Immamura & al. 2015 in the reference section). With regards to the DQI-I in our specific context, a study on Tunisian adolescents (Aounallah-Skhiri & al; 2001 in the reference section) showed that a “modern” dietary pattern was linked both to a decrease in the moderation component of the DQI-I (ie increase in foods/nutrients linked to a higher risk of chronic diseases) but also an increase in adequacy and variety of the diet. Also in our study, we showed that women vs. men contrasts depended on which dimension of diet quality was considered (and that suggest possibilities for gender specific prevention messages).

About the reliability and validity of the method, the mode of computation and reasoning for the cutoffs as described in the method section of our manuscript, the main founding article concerning the DQI-I is that of Kim S, Haines PS, Siega-Riz AM, Popkin BM in the Journal of Nutrition in 2003 (cf. in the reference section).

In accordance to the reviewer’s suggestion, in the methods section we have added detailed information on why we chose to characterize diet quality using the DQI-I score.

10. Lines 283-287 discuss the specific findings on the consumption of various foods in your sample. This does not appear to be directly relevant to the topic of the paper at hand. Please remove.

Authors’ response

If the reviewer would agree, we would rather keep that short paragraph which discusses the overall (women + men, with relevant post-stratification weights) consumption of food groups in this sample as the context of the study. Indeed although the focus of the paper is on gender differences, we would think these cannot be discussed independently of the overall food consumption context. Also we would consider that this paragraph, partly addresses one of the above comments of the reviewer to characterize the overall context regarding consumption of different foods pertaining to a more modern vs. a more traditional diet (e.g. white bread, pasta, soft drinks vs. fruits, vegetables etc.) in the context. Also these overall estimates somewhat address a point raised by reviewer 3 that the data is interesting beyond the analysis of women vs. men differences.
11. There are several references to studies from other countries, including an old study from the USA, that do not seem particularly relevant to the topic of the current study. Please remove these. In general, it would be more interesting if your discussion elaborated more on the implications of the main findings within the cultural context of Tunisia. It might also be helpful to describe the strengths, weaknesses, and future directions of your study in the discussion.

Authors’ response

Form our understanding, the reference of Kim & al. 2003 is the main article where the DQI-I was defined so that we would rather not remove it if the reviewer would agree. We have tried as much as possible to discuss the results in the Tunisian and/or more generally the MENA context: there are several instances in the discussion where we discuss how social norms, gender roles or social desirability could influence (or not) gender dietary contrasts (overall or in interaction with sociodemographic factors). On the other hand, as we underlined in the discussion, there are not many comparable food consumption studies in the context: we made reference to analogous but not entirely comparable data from Morocco and Lebanon. Also, about the reference to studies in completely different settings, we thought it could be interesting to contrast the gender differences in those context vs. that of our study.

Regarding the last point, the strengths, weaknesses and possible future orientations of the study are discussed thoroughly in the corresponding paragraph of the manuscript also supplemented according to the recommendations of the other reviewers in the revised version.

12. In the conclusion section and elsewhere, capitalize the DQI-I subscale names.

Authors’ response

This has been changed.

Reviewer #3

This is an interesting work with dietary intake data of 3-day food record from a population sample of more than 2500 people in Tunisia. The paper include some important information about diet quality of women and men 20–49 years of age. However, there are several points which need clarification and revision to be published in the journal.
1. Authors emphasize gender inequality and the problem of obesity in women in Tunisia which is not presented as a part of data in this manuscript. Therefore, problem of obesity and adiposity can be mentioned as a background of the research, preferably with concrete data from the country, but it is not appropriate to address obesity with regard to the results of the current paper, unless authors include BMI, or adiposity, of study subjects and relate the results to the BMI of participants.

Authors’ response

We understand the reviewer’s point of view. Cross-sectional studies have indeed major limitations regarding the assessment of diet-disease associations. So that it cannot be the purpose of the study to directly relate the adiposity data of the same subjects to their dietary intake. Also the adiposity data of these have been previously published as such. But indeed the gender obesity/overweight inequality is major in the context and that of course motivated the present study (as diet is known to be one key factors of nutritional status, including adiposity).

As for characterization of the context, in the introduction we mentioned both for the MENA region and Tunisia itself that there were large gender obesity inequalities. Following the reviewer’s suggestion, in the revised version we gave some detail regarding these women vs. men differences (cf. end of the introduction). From our point view we mostly kept the obesity issue as an element of context to discuss our results.

Following the reviewer’s suggestion in the revised version we also have removed e.g. line 274-281 (of the first submitted version) at the beginning of the discussion as well as several other sentences (e.g. in the conclusion).

2. It is possible that gender inequality of the society is an important contributing factor for dietary intake of women and men in Tunisia. Were factors of non-equalitarian household and social roles for men and women measured in the study? If so, they should be presented in data and their effect on DQI-I should be analyzed. Since only more general SES indicators, such as household size, education, and professional activity, are included in the analysis, it may not be appropriate to emphasize gender inequality in this paper.

Authors’ response

From our point of view, gender inequality can be indeed be assessed at various levels over a causal chain, which can range from more fundamental causes (e.g. socio-cultural, legislation) to intermediate socio-economic factors (such as education or professional activity) up to lifestyle (diet as in our study, physical activity level) or even health outcomes (as we have studied gender inequality regarding obesity in previous articles). Intermediate socio-economic factors such as
those used in our study can then be both: - indicators of gender inequalities (e.g. that women of this age range have similar level of education vs. men but a much lower rate of professional insertion and/or lower category of profession), - and drivers of within-household or social gender roles (which e.g. have been shown to be less unequal in the higher levels or education and especially of professional activity), -which then can impact lifestyle factors and health outcomes.

So that the aim of the paper would be to describe gender inequalities regarding diet (not excluding that they could be null or low), while studying whether they vary with socio-economic factors (by analogy with the interactional problematic of the social sciences, which aims at studying gender inequalities in different socio-economic strata, as stated in the methodology section). We also discussed how gender roles could be involved in differential gender contrasts between socio-demographic categories (e.g. age, size of the household or education).

3. In 'Design and sampling," it is a stratified cluster sampling of households, but there are large difference between women and men (1,689 vs. 930). What are response rates of women and men in the sample? If response rates are different considerably, there may be a big selection bias. Can authors look into response rates of women and men, and present some information about non-responding women and men and check whether there can be sampling bias, and if so, how they should be considered in interpretation of data.

Authors’ response

As suggested by the reviewer we added the response rates in the methods/subjects section (as reported in a previous article pertaining to the same sample of adults but different outcomes, the response rate was 89.5% for women and 67.7% for men). The lower response rate among men vs. women is not unusual in the context. Nevertheless, many studies in the LMIC context often pertain to adult women only (as they are considered a population at risk for a number of nutrition related pathologies) so that having dietary intake on a large sample of both men and women is nevertheless a strength of the study.

Of course we agree with the reviewer that bias issues should not be overlooked. The main point of the study was to compare women vs. men rather than derive estimates at population level but the lower proportion of men could be nevertheless an issue for such overall estimates. As stated in the methods section, beyond sampling weights which account for the differential probabilities of inclusion due to the complex sampling plan, the weights used in the analysis also integrate post-stratification on gender, age and place of residence (that is estimates are standardized to the last census estimates for the control variables). So that for example when computing overall mean energy of nutrient intakes or DQI-I over the whole sample, men were weighted so that their proportion is that of the target population (roughly ½ for that specific variable), and so on
for the other control variables. For the other socio-demographic characteristics, it stems from Table 1 that the weighted distribution of socio-demographic variables in women and men do not suggest a major selection bias among men (with respect to census data as e.g. available from the Tunisian National Institute of Statistics ins.nat.tn).

In keeping with the reviewer’s suggestion we nevertheless have underlined more that possible selection bias in the discussion section (strengths and limitations).

4. Since it is not common to have 3-day food record data from more than 2,500 subjects, it will be important to analyze about their dietary intake, diet quality and contributing factors for women and men in more detail, not just for difference between women and men, to derive gender-specific policy measures.

Authors’ response

The main focus of the paper is that of gender differences/inequalities, regarding diet with a focus on diet quality (with reference to the special issue of Nutrition Journal for which we contributed the paper). So that our main goal was to assess gender differences/inequalities using suitable measures as e.g. done for excess adiposity in (Robinson WR, & al.. Am J Clin Nutr. 2009 89: 1204-12 and also in the context of our study El Ati J & al. PLoS One 20127(10):e48153 or Traissac P & al. Public Health Nutr 2016, 19(8):1428-1437). We then first presented estimates of the gender inequality measures at population level e.g. table 2 for nutrients and table 3 for the DQI-I score and sub-components.

Then it was of interest to assess how these inequalities might vary according to a number of environmental, socio-economic and individual factors, by analogy to what, in the social sciences is referred to as “intersectional analysis” (how gender intersects with other determinants), such as referenced in the paper by Hammarstrom A, Johansson K, Annandale E, et al. Central gender theoretical concepts in health research: the state of the art. J Epidemiol Community Health. 2014 68: 185-90. and also as done by Robinson & al., El Ati & al, Traissac & al. as quoted above. For unadjusted analyses, for each covariate the estimates were derived from bivariate models where the response variable (either interval of binary) was modeled as a function of gender, covariate and gender x covariate. For adjusted analysis the model included gender and covariate and gender x covariate terms for all covariates (as we stated in the last paragraph of the methods sections).
Technically, the interactions terms are “symmetric” from a statistical point of view, i.e. with the same models, the same parameter estimates and the same P-value, one can “detail” a gender x covariate interaction either:

a- by assessing how the relationship between the covariate and the response variable varies between genders, that is assessing the modifying effect of gender on the relationship between diet quality and covariates (which is what the reviewer suggests).

b- or by assessing how the “relationship between gender and the response variable” (i.e. the measure of gender inequality) varies between the different levels of the covariate. That is assessing the modifying effect of covariates on the associations between gender and the response. This is what we did, as our main interest lies in measures of gender inequalities and how they vary in the different levels of the covariates: so as to be able to identify e.g. that gender inequalities decreased with age, or increased with size of the household, or varied (with an inversed U shaped relationship) with level of education etc.

These two interpretations, though arising from the same data and models parameters, thus correspond to a different strategy. So that the “symmetric” (a) presentation of the results suggested by the reviewer (which would use exactly the same model and parameters) would somewhat address differently the problem and would seem less relevant to our gender inequality issue focus than the (b) we chose. Indeed our focus is not on the direct association of covariates with diet quality, but on how the covariates modify (or not) gender inequalities. Having both types of detailed results presented in the paper would to our opinion run the risk of being redundant or confusing the reader.

5. If body size, was measured from study subjects, and if authors want to relate DQI to obesity problem, height, weight, and calculated BMI should be included in data, and should be included in data analysis.

Authors’ response

Cf. comment above, gender inequalities regarding corpulence and/or excess adiposity are quoted as the context of the study (data pertaining to gender differences in adiposity and iron deficiency and/or anemia on the sample have already been published, in Traissac & al. Public Health Nutrition 2016 quoted in the reference section of the paper). It is beyond the objectives of the
paper to assess associations between diet quality and BMI, also as cross-sectional studies have indeed major limitations regarding the assessment of diet-disease associations.

6. Sociodemographic factors (Table 1) of women and men in the study are similar except for education and professional activity. It is interesting women have more university education than men (22.2% vs. 17.2%). Is it true for general population? If not, it may be from sampling bias and need to be considered in data analysis. For professional activity, difference between women and men are so huge, one can wonder whether it is appropriate to include this factor in multivariate model. It may also be related to education, especially among men. It is an important information about population characteristics but authors may consider deleting this factor or find suitable way to classify the factor to use in data analysis.

Author’s response

If the reviewer would agree, there would not seem a major issue that the distribution of professional activity is quite different between women and men (on the contrary that would seem to underline the importance of including this factor in the analysis).

Indeed it is a characteristic of the context of the study that due an emphasis on education policies and also social norms rather less detrimental to women than other countries of the region, women and men have overall quite similar levels of education (at least in this age range and this mostly urban context). Nevertheless a consequence of not yet gender neutral social roles is that, at overall equal levels of education, the rate of professional activity is much lower among women (and also towards less qualified jobs).

In previous studies (e.g. in Morocco and Tunisia), that we quoted in the reference section, it appeared that regarding gender differences in health outcomes professional activity is of foremost importance. These studies showed that a higher level of education is rather not sufficient to lower gender inequalities in health if it is not translated into a professional activity outside the home. Working outside the home seems foremost in promoting empowerment of women within and outside the household, and for more egalitarian households and social roles with beneficial associations towards less excess adiposity among women, through a variety of mediating factors (e.g. such as less sedentary behavior, decreased food stimuli or slimmer body image models etc.). So that it would seem of foremost importance in our “intersectional” quantitative analysis of gender differences in diet quality, to also assess how these differences
varied by levels of professional activity (even if they were eventually found not to vary markedly, or at least less than between categories of some other cofactors).

7. In Table 2, please include energy as % requirements in the table. Apparently authors calculated it (LL205, p.9) but did not present clearly in the table. Mean value was 104%, significantly higher than 100, but did not present it by sex. It will be an important indication about obesity problem of the population, especially if women had higher value than men. But if there was no difference, it is still important information with regards to obesity. Usually macronutrients are presented as % energy, rather than intake per 1,000 kcal.

Author’s response

As suggested by the reviewer, energy as % requirements was included in Table 2. The comparison between women and men is addressed in detail in the discussion section of the manuscript.

Regarding use of the nutrient density model (g/1000 kcal), cf response to point #8 of the reviewer below (as this also applies to macronutrients). But we agree with the reviewer that macronutrients in % of energy is also a useful complementary information so that it is presented in the text of the results section of the submitted manuscript.

8. For food intakes (Figure 1), it will be easier to compare women and men if they are presented together in the same direction, but with different color than in the current graph. For foods and food groups, absolute intakes are frequently more of interests than per 1,000 kcal.

Author’s response

The figure 1 has been changed accordingly.

Adjusting for energy intake (e.g. using the nutrient density model in g/1000 kcal as in our study) is considered standard practice in nutritional epidemiology especially when one is interested in the composition of the diet than the total amount food. Whatever the specific context, and for a variety of reasons, women and men have generally very different overall food consumption (in absolute quantities) and also total energy intake. So that analyzing absolute amount of foods would not being entirely meaningful and would mostly result in un-informative systematic differences towards smaller amounts for women vs. men. Also, an interesting side effect is that this adjustment can also significantly reduce measurement errors: both those due to extraneous
variation in intakes related to body size, physical activity and metabolism but also technical error due to the dietary assessment instrument. (Willett W: Implications of Total Energy Intake for Epidemiologic Analyses. In: Nutritional Epidemiology. Edited by Willett W. New York, USA: Oxford; 2013:160-286), which was listed in the reference section of the manuscript.

9. For statistical test for women and men difference in Table 2, were the differences adjusted for significant SES factors - education, and professional activity if authors decide to keep the factor with appropriate adjustment of categories? If so, please specify it in methods section.

Author’s response

Results for table 2 (and 3) are for descriptive purposes. We have added a precision in the footnotes regarding the model used for deriving the women vs. men (unadjusted) estimates.

10. Comparison of DQI-I of women and men are interesting. (Table 3, 4 & Figure 2) I recommend to conduct analysis of diet quality index and subcomponents in women and men separately rather than analyzing for difference only.

Author’s response

Cf. detailed answer to reviewer’s comment #4 pertaining to the same analysis issue.

11. In data analysis, using too many variables in multivariate model can be dangerous. In this study sample, only education (and professional activity) was significantly different between women and men. Did authors try to analyze with adjusting for only significant variable?

Author’s response

We agree with the reviewer that using too many regressors in a model can be an issue for a variety of reasons.

It can be so if the number of parameters of the model is too high vs. the number of observations: an usual threshold is that the number of parameters should be <10% the number of observations. Even our final adjusted model including interactions with all socio-demographic
It can be so because of too much collinearity between the regressors, which can result in inflation of the variance of the estimates and thus uninformative confidence intervals. So that it is important to provide estimates of unadjusted (or bivariate) associations to compare with adjusted ones also for this purpose. The bivariate/unadjusted associations of the regressors with the different categories of the response variables were presented in Table 4 of the submitted manuscript, (c.f. the crude diff or OR columns). There were not significant inflation of variance of the estimates from the unadjusted to the adjusted analysis: this would have resulted in much larger confidence intervals for adjusted parameters vs. the unadjusted ones (which we did not observe, cf Table 4). So that collinearity issues can probably be ruled out.

Also, beyond those entirely technical issues, some authors would recommend including or not variables in a multivariable model, based more on reasoning and/or a conceptual model (which includes expert knowledge on the issue/context) than data driven selection criteria (which are sensitive to sampling variation). This would be so for a variety of other reasons, including better assessment of confounding.

12. Analyses of DQI-I > 60 in Table 4, seem to make the results more confusing, and are recommended to delete in the table.

Author’s response

If the reviewer would agree we would rather keep the data regarding “good diet quality (DQI-I>60)” binary variable. It would seem frequent practice to run in parallel analyses pertaining to the same dimension as interval variable and coded as a categorical variable (e.g. yes/no binary variable using a relevant cut-point as in the current analysis). Examples are BMI and obesity (BMI>=30), hemoglobin level in g/L and anemia etc. This would seem to both provide complementary information and also some sort of “sensitivity” analysis (robustness of the analyses when using different coding of the same measurement and thus here also different quantifications of gender diet quality inequalities).

13. Contents presented in the table need not be repeated in the text, especially in 'Results' section.

Author’s response
We have deleted some of the numerical results which were dispensable in the text of the results section.

14. In 'Discussion' too much emphasis is given to excess obesity (and adiposity) in women which is not directly analyzed in this study. Also, gender inequality is not directly analyzed with the study subjects. It will make the paper more clear to limit the discussion what was actually measured and analyzed in the study.

Author’s response

Cf. response above to reviewer’s comment #1 about the same issue. We think it is important as the context of the study and the motivation underlying the study.

In keeping with the reviewer’s suggestion we have shortened or deleted some parts of the discussion pertaining to that matter.

15. In 'Conclusion', what was said in results and discussion should not be repeated but what authors found out with regard to the purpose of the study and the implications for policy or future study need to be concisely addressed. This part needs to be re-written.

Author’s response

This has been changed and the conclusion has been significantly shortened, especially to avoid unnecessarily repeating the results.