Author's response to reviews

Title: Nutrition transition among adolescents of a south-Mediterranean country: dietary patterns, association with socioeconomic factors, overweight and blood pressure. A cross-sectional study in Tunisia.

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Version: 3 Date: 7 February 2011

Author's response to reviews: see over
Subject : revised version of manuscript  MS 7713576644819213

Dear Editor,

You will please find as on-line submission the revised version of the paper entitled

Nutrition transition among adolescents of a south-Mediterranean country: dietary patterns, association with socioeconomic factors, overweight and blood pressure. A cross-sectional study in Tunisia.

by

Hajer Aounallah-Skhiri, Pierre Traissac, Jalila El Ati, Sabrina Eymard-Duvernay, Edwige Landais, Noureddine Achour, Francis Delpeuch, Habiba Ben Romdhane, Bernard Maire

Reference MS: 7713576644819213

We thank both reviewers for taking time to thoroughly review the manuscript; we have tried our best to answer their comments and/or make the suggested changes or additions.

Each author has read and approved the content of this revised version. The submission represents original work that has not been published previously and which is not currently being considered by another journal.

We would be pleased that this revised version be accepted for publication in Nutrition Journal.

Yours sincerely.

Pierre TRAISSAC
Authors' response to reviewer Hélène Delisle

Reviewer's report

Title: Nutrition transition among adolescents of a south-Mediterranean country: dietary patterns, association with socioeconomic factors, overweight and blood pressure. A cross-sectional study in Tunisia.

Version: 1 Date: 4 December 2010

Reviewer: Hélène Delisle

Reviewer's report:

• General comments
The paper addresses a relevant topic, nutrition transition in adolescents. It is well written (although some further English editing may be advisable), statistical methods are impressive and the illustrations elegant, data analysis is thorough, and the reference list is complete. The “modernization” scale is an interesting finding. The paper is however fastidious to read, as it is very long with lengthy description of statistical procedures, too many results and tables overly packed with data. As a consequence, the salient findings of the study are lost.

Authors' response: we thank the reviewer for taking time to review the manuscript and the appreciative comments above.

1. We strongly recommend that the paper be shortened and the main results highlighted.

Authors’ response:

Regarding overall length of the manuscript, it somewhat derived from our aim of describing food consumption of Tunisian adolescents from different perspectives; indeed although emphasis is on observed dietary patterns, it seemed difficult from our point view not to present a minimal amount of descriptive dietary data (within the limit of the FFQ instrument) as well as socio-economic context. Also the instructions for authors are quite liberal regarding the total word count. Nevertheless, as suggested by the reviewer we have tried to simplify the information displayed (e.g. by suppressing table 3 presenting detailed information related to association of dietary scores with socio-economic characteristics; although basic socio-economic data has been kept in a separate table to the requirement of another reviewer).

We also acknowledge descriptions of statistical procedures were sometimes lengthy in the first version of the manuscript, and as suggested by the reviewer we have suppressed non essential information and rewritten/shortened the redaction pertaining to analysis procedures in the methods section as well as in the part of the discussion section dealing with methodological issues (cf. relevant highlighted sections in revised manuscript: page 5, lines 1-10; page 6, lines 19-35; page 14 lines 4-10; page 16, lines 7-19)

• Minor essential revisions
2. The authors do not discuss the use of DQI-I, which is based on USA recommendations, whereas they refer to WHO recommendations (ref 51).

Authors’ response: Our main objective was to compare dietary intake of Tunisian adolescents to international references.

As for reference for absolute intakes of nutrients, WHO references seemed the more relevant for international comparisons.

As for diet-quality, DQI-I (which includes reference to US and Chinese recommendations) was used to characterize diet quality “in a standardized way that enables cross national comparisons” (especially those undergoing a nutrition transition situation) as discussed in the paper by Kim & al J. Nutr. 2003. It may be of course that this index is not as all-purpose as discussed by Kim (e.g.: Tur JA, Romaguera D, Pons A: The Diet Quality Index-International (DQI-I): is it a useful tool to evaluate the quality of the Mediterranean diet? Br J Nutr 2005, 93:369-376).
We did not elaborate much further on the matter given that although it does provide useful complementary information not available elsewhere, data regarding DQI-I is nevertheless relatively of secondary importance in the study, either in the descriptive part or regarding dietary patterns.

3. Data were collected in 2005: further changes may have occurred since.
   **Authors’ response:** A sentence has been added in the discussion section regarding this likely evolution (page 15, line 35-37).

4. Description of statistical methods is very long.
   **Authors’ response:** as explained above we have tried our best to remove non essential information (cf new version of the manuscript).

5. Were the rejected subjects (N~200) in any way different from those retained for the analyses?
   **Authors’ response:** missing data was an issue mostly due to total non response among males for which the response rate (about 70%) although quite high compared to male response rates in other studies in the same context, is markedly inferior to that observed for women (>90%); socio-economic data by gender presented in Table 2 (added to the requirement of a second reviewer) in the new version of the manuscript show that there was likely no major selection bias for males. Also, in the new version of the manuscript, selection bias is addressed in the discussion section (page 16, line 4-6).

6. Under “Food consumption and physical activity”, para 1: “total sugars” refers to free sugar, not total carbohydrate.
   **Authors’ response:** yes, and ‘total sugars’ has been replaced by ‘free sugar’.

7. There is no information on how salt and spices were quantified.
   **Authors’ response:** Salt and spices assessment was based on the food frequency questionnaire (FFQ). As stated in the methodology section the FFQ itself was adapted for adolescents from a FFQ specific to the Tunisian context which was validated vs. a 3 day record method (ref 27 in first version of the manuscript : El Ati J, Le Bihan G, Haddad S, Eymard-Duvermay S, Cherif S, Holdsworth M, Traissac P, Ben Rayana C, Delpeuch F: Food Frequency Questionnaire for Tunisian dietary intakes : development, reproducibility and validity. Arab Journal for Food and Nutrition 2004, 5:10-30.).
   Data on food and dishes consumption of adolescents obtained from the FFQ were then converted into nutrient intakes from the specific Tunisian food composition table, for which food preparation methods and ingredient portion sizes of dishes were previously collected by dietary surveys and their composition was calculated using recommended methods (ref 28 in first version of the manuscript : El Ati J, Béji C, Farhat A, Haddad S, Chérif S, Trabelsi T, Danguir J, Gaigi S, Le Bihan G, Landais E, et al: Table de composition des aliments tunisiens. Tunis, Tunisia: INNTA (National Institute of Nutrition of Tunisia); 2007.)

8. In Fig 2, spices, other condiments, harissa, tea, coffee and water could be excluded, which would make the figure more legible. We question the relevance of including these as “food groups”.
   **Authors’ response:** We agree with the reviewer that there maybe a vocabulary issue regarding “food group” when referring to those items : as a matter of fact the 43 items presented in figure 3 results from “groupings” of the 134 items of the FFQ, based on the literature and knowledge pertaining to the Tunisian context (as presented in table 1). Indeed, some of these “food groupings” do correspond to definition of a “food group”, some others are composed of a single food or item so that we could have used a different word to designate those 43 regrouped items. Nevertheless, we used the term “food group” for lack of better vocabulary and by analogy with other published studies.
   We would tend to think that the information presented (in fact in figure 3) is useful as most of these items are somewhat emblematic of dietary practices in arab countries or even more specific to Tunisia. For example, although we did not elaborate on the matter in the manuscript it is interesting to remark that consumption of harissa somewhat paradoxically increased along the
“modernisation” gradient: it is indeed a traditional preparation but which happens to be also very often consumed when eating out of home (either as a side dish with a seated meal but also most always added to the variety of sandwiches sold in fast food outlets). On the other hand consumption of a traditional beverage such as tea steadily decreases along the modernisation gradient as it likely tends to be replaced by other types of beverages (e.g. mostly sodas and sweetened beverages). Therefore, if the reviewer would agree with us we would rather keep that information in figure 3.

9. Under “results”, “modern dietary pattern score”, it is suggested to describe the first axis as the modern pattern score early on, rather, than in the last paragraph of this section.

Authors’ response: This has been changed as suggested by the reviewer, the sentences defining the “modern” axis has been moved to beginning of the paragraph. (cf. Page 8, Line 31-34)

10. The second axis, named “Meat-fish pattern”, does not appear as well defined as the first axis, which can be interpreted as a scale of “modernization” of the diet. Is the second axis of any interest?

Authors response: from the criteria most often used in dietary pattern studies to assess relative importance of components extracted by methods such as PCA or MCA (scree plot of eigenvalues), there were distinctively two main axes (of decreasing relative inertia), cf. figure included. Indeed when dealing with dietary patterns one must put an emphasis on interpretation of the axes regarding not only statistical criteria but also mostly dietary intake either as food groups and/or nutrients and/or diet quality.

As for the second axis it was very specifically characterised by an increase in consumption of meat and fish products and also somewhat of olive oil as well as a (milder) decrease in some of the food groups emblematic of the “modern” diet score. It featured a monotonous increasing relationship with energy, iron, zinc, vitamin B12 and it revealed a gradient of diet increasingly and linearly associated with variety, adequation and balance sub-scores of the DQI-I resulting in overall increasing total diet quality (contrary to the modern pattern for which diet quality leveled off or even decreased for the higher values of the score).

From our point of view, its interest thus lies in that it shows a different path to a modified diet with a regular increase of diet quality, mainly linked to consumption of animal food, which is known to raise a number of issues both from a nutritional but also environmental point of view.

11. In view of the inverse association of the modern diet score with blood pressure, it would have been interesting to also show intakes of zinc, folates and vitamin B12.

Authors’ response: as suggested by the reviewer the information regarding those specific nutrients has been added in table 3, in figure 4 as well as in the results and discussion sections.

12. In the abstract results, the third sentence is unclear, beginning with “vegetables”.

Authors’ response: this has been changed.

• Minor corrections

13. In the section “Food consumption and physical activity, reference 48 is wrong

Authors’ response: we have checked the references in the new version of the manuscript.


Authors’ response: this has been added (page 12, line 35).

15. Discussion, para 4, line 3: FOOD and not nutritional point of view

Authors’ response: this has been changed (page 13, line 30).
16. Table 2: Physical activity level should be in a separate table. Footnote 2 is unclear.

Authors’ response: if the reviewer would agree with us we would rather keep this information regarding physical activity in that same table as this would otherwise add one more table to the paper (also taking into account that dietary intake and physical activity are both proximal determinants of anthropometry and B.P. so that it may make sense to present descriptive data regarding those two factors together).

Footnotes have been reworded.

17. Table 3: too much data for one table. Please select the most meaningful. Footnote 4 is unclear.

Authors’ response: table 3 has been deleted. Essential results regarding associations between dietary patterns and socio-economic factors are given in the corresponding paragraph in the text. Distribution of socio-economic factors in the sample are now in new table 2 (as suggested by another reviewer).

18. Fig 1: “socio-cultural psychological factors”: is this circle supposed to be there?

Authors’ response: the circle is not formally included in the conceptual framework in the same way as the other factors, both to indicate that the factors in the circle interact with the others at the different levels and also that they are not formally taken into account in the study but should nevertheless be discussed. We have slightly modified figure 1 (added arrows) in the new version of the manuscript so as to better underline this point.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Authors’ response: The new version of the manuscript has been proofread by a native English speaker.

Also regarding vocabulary, there was a small inconsistency in the first version of the paper as high-blood pressure (as defined in the methodology section and used in the results and discussion sections) was sometimes referred to as “pre-hypertension” (e.g. in the abstract or in table 4): in the revised version changes have been made (abstract line 25, in table 4) to use consistent terminology throughout the paper.

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: I declare that I have no competing interests.
Authors’ response to reviewer Federico J Perez-Cueto

Reviewer’s report

Title: Nutrition transition among adolescents of a south-Mediterranean country: dietary patterns, association with socioeconomic factors, overweight and blood pressure. A cross-sectional study in Tunisia.

Version: 1 Date: 5 January 2011

Reviewer: Federico J Perez-Cueto

Reviewer’s report:
The article entitled “Nutrition transition among adolescents of a south-Mediterranean country: dietary patterns, association with socioeconomic factors, overweight and blood pressure. A cross-sectional study in Tunisia.” presents the results of a three regions representative nutrition study of Tunisian adolescents. As it is, it is a pertinent contribution to the nutritional knowledge; the work is original and performed in a sufficiently large sample. It has identified two major dietary patterns of Tunisian adolescents that are relevant for public health nutrition purposes. Methodologically, it is innovative in the use of MCA for the identification of dietary patterns. I have however some comments to the document.

Authors’ response: we thank the reviewer for taking time to review the manuscript and the appreciative comments.

Major Compulsory Revisions

1. Authors should better justify why they have not used the IOTF BMI-for-age cut-off points (Cole et al 2000). Although they mentioned a short communication by Must & Anderson, 2006, there is room for proper discussion on the use of such references, their bias and how to draw conclusions from national references. As example, the article by Baya Nutr Hosp. 2010;25(3):428-436 presents a recent discussion on the issue; authors may want also to refer e.g. to one of the first papers on the matter such as Wang & Wang 2002 (EJCN 56:973–982) or the series of papers by the group of Neovius (e.g. Obes. Rev. 2004;5:105–114).

Authors’ response: We thank the reviewer for the useful references. As a matter of fact, the present study is based on a subsample of a national survey, whose primary aim was anthropology and association with socio-economic factors (Aounallah-Shkiri H, Romdhane HB, Traissac P, Eyamard-Duvermay S, Delpuech F, Achour N, Maire B: Nutritional status of Tunisian adolescents: associated gender, environmental and socio-economic factors. Public Health Nutr 2008;1-12.) i.e. ref. # 22 in the first version of the submitted paper (now ref #23). Issues regarding choice of references were discussed in this paper and as, at the time, there was no clear evidence on which reference would be better, it was decided to use the WHO reference for the main results (although some figures were also given using IOTF reference); so that we also used the same WHO reference in the present manuscript if only for comparability with these already published results (also taking into account that anthropometric data was not the main focus of the study). But this is indeed a relevant issue and as suggested, in the new version of the manuscript we have addressed it in the discussion section (page 16, line 34-37; page 17, line 1-2 and new references #83 to 85).

2. How was post stratification weighting performed? How did authors calculate weighting factors? Could the lower response rate in males be a source of bias in the results? Please address the latter in the discussion section.

Authors’ response: Weighting factors used in the analyses are the product of : - sampling weights (inverse of selection probability), to take into account differential probabilities of selection deriving from the stratified/clustered sampling, - poststratification weights, which partially attempt to correct for non response by adjustment here (by region) on sex, age and area (urban vs. rural), so that the sum of weights of the sampled subjects matches “control counts” (here from the 2004 national population census). Ref. Korn EL, Graubard BI: Analysis of health surveys. New York: John Wiley & Sons.; 1999.
Adjustment of estimates for non-response by such weighting, if it can be useful for certain purposes (e.g. correction of the gender imbalance when estimating parameters over the whole sample) would of course not solve major selection bias issues. In the present study the response rate is indeed somewhat lower for males vs. females but at least from the point of view of socioeconomic characteristics (cf. Table 1 in the new version of the manuscript), there are hints of no major bias issues. Indeed, this issue of selection bias is central in cross-sectional studies and, as suggested by the reviewer, we have now addressed it in the discussion section of the revised manuscript (cf. page 16, line 4-6).

3. Food Frequency Questionnaire: The advantages and limitations of FFQ are well known. Therefore, and to enhance the paper, authors should describe better the instrument (Page 4, lines 19-31). How long was the recall period? Was it over the past week, month or year? This has furthermore implications, particularly for the estimation of nutrients. Probably at population level individual variation might be mitigated (particularly by the large sample size), but results although well presented (Pages 7-8) should be properly discussed (lines 13-18, page 16) against the particular limitations of the instrument.

Authors' response: Length of the recall period was stated page 4 line 19 “One month retrospective food consumption…” in the first version of the manuscript. As suggested by the reviewer we have discussed some more the issues regarding use of FFQ in the discussion section (page 16 , line 21-29).

Minor Essential Revisions
In the introduction, authors may want to add the paper by Al Sabbah (Public Health Nutrition 2007;10(7):739-746) to the evidence of previous works done in adolescents of the Arab world.

Authors' response: We thank the reviewer for the useful reference which has been added to the new version of the manuscript (page 3, line 18, ref #19).

Furthermore, it would be helpful to know the proportion of the Tunisian population that is in the age of adolescence. I think it will make the study even more relevant.

Authors' response: In 2005 when the study was carried out, in Tunisia there were about one million 15-19 y. adolescents i.e. about 11% of the whole Tunisian population, this age-class being the most numerous due to the narrowing of the base of the age pyramid as part of the demographic transition, while the 10-19 y. totaled about 21%. Thank you for the very relevant suggestion and the corresponding figures have been added in the introduction in the new version of the manuscript (page 3, line 13-16).

Proxy-indicators of socio-economic status should be better elaborated. Although self reference is made to previously published papers, it would be adequate if authors could mention other groups (or statistical offices) that use such measurements. Furthermore, experience has shown that Education is one of the best proxy-indicators of SES (as suggested e.g. by Liberatos et al 1998, Epidemiol. Rev. 10, 87–121), so why not to focus on this indicator alone? Present the sample characteristics (sociodemographics) in one table. It will enhance the overall picture portrayed by this study. Table 3 is quite large, and though it contains the sample composition, it would be better to have it in one table apart and at the beginning of results section.

Authors' response: We agree with the reviewer that estimation of socio-economic status (either of the household and/or the subject itself) is of importance in the general context of health studies either as a main focus or for adjustment purposes (although not central in the present study). As a matter of fact, in line with some authors (e.g. Braveman PA, Cubbin C, Egerter S, Chideya S, Marchi KS, Metzler M, Posner S: Socioeconomic status in health research: one size does not fit all. JAMA 2005, 294:2879-2888) we would tend to assume that SES is not always one dimensional so that (especially when studying associations with health outcomes) it should be important to distinguish between different dimensions e.g. such as income/wealth and education (of the head of household but also the mother and the subject her/himself). Indeed, in a number of studies, have been observed independent associations of education and wealth with the studied outcomes (including this one, c.f. adjusted associations and thus independent effect of household wealth and education with the dietary patterns). Turell et al. have shown for instance that diet could
be influenced distinctly by dietary knowledge on the one hand, by food costs on the other hand; although both aspects may converge in low socio-economic households, they may not have the same loading in a number of instances (Turell G, Kavanagh A (2006). Socio-economic pathways to diet: modeling the association between socio-economic position and food purchasing behavior. Public Health Nutrition: 9(3), 375–383.).

As for the more technical point of estimating the specific sub-dimension of household wealth, so called “assets based methods” as proxy of income or expenses, derived from principal component analysis or similar methods, have been used by other authors (e.g. Wagstaff A, Watanabe N: What difference does the choice of SES make in health inequality measurement? Health Econ 2003, 12:885-890); indeed, as choice of items to include in the proxy do depend on the particular context, we tended to include reference pertaining to construction and use of such proxy in the same context as that of the present study. As suggested by the reviewer, so as to avoid exclusive self referencing, the above reference by Wagstaff has been added in the new version of the paper (ref. #24).

Also as suggested by the reviewer, in the new version of the manuscript there is now a separate table for socioeconomic characteristics (new Table 2). N.B.: also due to advice from another reviewer to focus on main results, table 3 from the first version of the manuscript has been removed, results regarding association with socio-economic factors being given only in the text of the results section.

In Table 2, authors present unadjusted values for nutrient intake. Since Energy is one of the major sources of bias, it would be adequate to present energy-adjusted values.

Authors’ response: In table 2 (of the first submitted version, which is now table 3 in the revised version), macro and micro nutrient intakes are presented in g/1000 kcal/ day, which is (among others) one way of adjusting for energy intake (nutrient density model) : Willett WC, Howe GR, Kushi LH: Adjustment for total energy intake in epidemiologic studies. Am J Clin Nutr 1997, 65:1220S-1228S; discussion 1229S-1231S (ref. # 35 in the first version of the paper).

Table 3 should be amended accordingly if authors would include the suggested table with sample characteristics.

Authors’ response: This has been modified accordingly (cf. in the revised version the newly included table 2 presenting socioeconomic characteristics of the subjects).

Discretionary Revisions:
1. Authors may want to enhance their literature review and place it in a more global context of nutrition transition. What is the weight of out of home eating in Tunisia?

Authors’ response: from 2005 expenditure data, % of food expenditures/ total expenditures was 34.8% while out of home eating represented 5.3% of total expenditures, hence about 15% of total food expenditures (Institut national de la statistique: Enquête nationale sur le budget, la consommation et le niveau de vie des ménages en 2005. Tome I : Les dépenses. pp. 206. Tunis; 2007). Nevertheless, these figures are quite indirect and very macro estimates, which we are not sure are entirely relevant in the context of the present study ?

2. Authors may want to elaborate on what they mean by “trained personnel” for the anthropometric measurements.

Authors’ response: The point is indeed relevant but we would not want to elaborate on all measurements procedures given that the manuscript is already quite lengthy and also that anthropometric data is not the main focus of the paper. Nevertheless, as suggested by the reviewer we have added one sentence regarding that issue in the methods section (page 5, line 26-27).

3. Page 16, line 1: Sentence not clear, to many ‘with’ (probably a typing mistake).

Authors’ response: As a matter of fact, in the new version of the manuscript the paragraph has been rewritten/shortened so as to take into account comments from the second reviewer but thank you for pointing out the typing mistake.
Language:
The text should be revised to improve clarity. Punctuation should also be revised throughout the text. Single example, Page 12, lines 12-14:
For the meat-fish diet score, although before adjustment for energy intake and BMI and WC, there was an increase in diastolic blood pressure no major associations were observed either. Here a comma is missing after ‘associations’ while the one after WC isn’t needed. The text could be further changed into:
Although an association with increased DBP was observed before adjustment for energy intake, BMI and WC, no associations with dietary patterns remained significant after controlling for their effect.

Author’s response: The new version of the manuscript has been proofread by a native English speaker with special emphasis on punctuation.
Also regarding vocabulary, there was a small inconsistency in the first version of the paper as high blood pressure (as defined in the methodology section and used in the results and discussion sections) was sometimes referred to as “pre-hypertension” (e.g. in the abstract or in table 4): in the revised version changes have been made (abstract line 25, also in table 4) to use consistent terminology throughout the paper.

Level of interest: An article of outstanding merit and interest in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests: I declare that I have no competing interests