PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Low income is associated with poor adherence to Mediterranean Diet and higher prevalence of obesity: cross-sectional results from the Moli-sani study
AUTHORS	Iacoviello, Licia; Bonaccio, Marialaura; Bonanni, Americo; Di Castelnuovo, Augusto; De Lucia, Francesca; Donati, Maria Benedetta; de Gaetano, Giovanni

VERSION 1 - REVIEW

REVIEWER	Michel de lorgeril, MD School of medidine of the Grenoble University France
REVIEW RETURNED	27-Jul-2012

THE STUDY	Regarding the evidence of health benefits of the Mediterranean diet, it should be fair to cite the unique randomised trial having tested it, for instance the final report of the Lyon Diet Heart Study (in Circulation in 1999) unambiguously showing strong protection against CVD complications. Results of the trial were then confirmed by several observational studies; important to underline however that randomised trials represent a better evidence than any meta-analysis of observational studies. In any case, both the trial and observational studies showed very similar benefits (also for cancers) which is rarely the case in nutrition epidemiology; see for instance the "antyioxidant story" ... Regarding the real cost of adhering to the Mediterranean diet model, it should be important to also discuss (in addition to the prices of the foods) the health economic advantages of reducing the rate of CVD complications. It would be fair for instance to cite and discuss the following article (even if it was in the context of secondary prevention): "A mediterranean diet is cost-effective in patients with previous myocardial infarction." By Dalziel K, Segal L, de Lorgeril M. In J Nutr. 2006 Jul;136(7):1879-85.
GENERAL COMMENTS	This is an excellent article raising the major issue of the relations between diet quality and socio-education status. I encourage the publication. Minor points: - about education level, why only two categories? It seems restrictive ... - use of three different parameters to evaluate the Mediterranean diet pattern: this is obviously a strength of that study! However, it would be interesting for many readers to explain why each of these parameters was not satisfying and why the three are needed; in other words, please, explain the weakness of each parameter! - there are more women in the low income category. It is not surprising but most middel-aged women are married and their own income (additional salary) could not be representative of their real

	income? Is the marital status important to include as covariable? Did you do it? - Very low current smoking rate in all categories: reporting bias? - Very high hypertension rate (>50%) in all categories: true or bias? Explanations? - Alcohol intake quite high in all categories: true? - No data on wine intake? - No data on oils other than olive oil? - No data on fish species? Fatty fish? - No data onother sea foods very characteristic of the Mediterraneans? Octopus?

REVIEWER	Saverio Stranges, MD, PhD Associate Professor of Cardiovascular Epidemiology Statistics & Epidemiology Division of Health Sciences University of Warwick Medical School Medical School Building Gibbet Hill Campus Coventry CV4 7AL (UK) I do not have any competing interests with the present manuscript.
REVIEW RETURNED	27-Jul-2012

THE STUDY	The English needs some revision. Some sentences were unclear, with a few grammatical errors throughout the manuscript.
GENERAL COMMENTS	This is an interesting manuscript from the Moli-sani project, a large, well-conducted epidemiological study from Southern Italy. The study addresses an important issue reflecting a rapidly evolving scenario. Specifically, the authors examined cross-sectional associations of socio-economic status (i.e., income and education) with adherence to a healthy Mediterranean dietary pattern as well as with obesity prevalence. This is an important public health issue given the on-going epidemiological transition in several countries of the Mediterranean region, namely southern Italy and Greece, which were traditionally associated with healthier lifestyles and dietary patterns than those observed in Northern European and American populations. However, recent evidence suggests that these countries have been gradually abandoning their original dietary patterns, at least in disadvantaged population subgroups, as a result of major societal changes and increasing financial constraints over the last few decades. Hence, there has been an escalating prevalence of major CVD risk factors in these populations, primarily obesity and hypertension, as well as suggestive evidence of leveling off or potential reversing of CVD mortality trends in these settings. As expected, the authors found that higher income and education were associated with a greater adherence to a Mediterranean dietary pattern and with a lower prevalence of obesity. These results reiterate the central role of socio-economic status in the adoption of healthy lifestyle and eating behaviours, which will eventually translate in lower morbidity and mortality from major chronic, diet-related diseases. These findings are of public health importance and further emphasise the urgent need for preventive policy measures, which have the potential to tackle the escalating prevalence of major CVD risk factors in these populations.

Major strengths of the present manuscript are the large, communitywide sample, as well as state-of-art measures for several of the variables included in the analysis.

However, there are some specific concerns and questions with the present study, which should be addressed to further improve the overall merit and clarity of this manuscript.

Specifically:

- Abstract: I would suggest rephrasing the objectives of this study with a sentence like "to examine cross-sectional associations of socio-economic status (i.e., income and education) with adherence to a Mediterranean dietary pattern and obesity prevalence". This would be in line with the title.
- Abstract: please specify that income refers to individual rather than household income.
- Abstract: it seems that income is associated with dietary patterns in both higher and lower education subgroups (table 5). This is an important observation, which should be highlighted in the abstract.
- Abstract: I would suggest removing the last sentence of conclusions "The increasing prices of the basic Mediterranean food items seem to represent a real obstacle to healthy diet driving people to choose alternative ways of eating usually inspired by the need to save money in everyday life." This is likely to be true. However, this speculative statement should be placed in the discussion only, because there are also other likely explanations for this changing scenario. In addition, given its observational, cross-sectional design, the present study cannot definitely prove this notion.
- Introduction: the authors should also cite some recent evidence from other Italian studies, supporting the notion the southern populations have unfortunately lost their advantageous "Mediterranean" cardio-metabolic risk profiles (see for example: Laccetti T et al. Public Health Nutr. 2012;30:1-11).
- Methods: despite the cross-sectional nature of the present analysis, we are aware that there is an on-going prospective follow-up in the Moli-sani project. The authors should clarify this aspect to justify the use of the terminology "cohort study" when referring to this project.
- Methods: the large number of missing data regarding income is obviously a potential limitation of this analysis, and may raise the potential of selection bias. The authors have fairly acknowledged this issue in their discussion.
- Methods: participants with prevalent CVD (5.7%) were excluded. Can the authors clarify what type of CVD fall in this category? Also, it seems that diabetics (6%) were excluded. However, in the paragraph on data collection, definition of diabetes diagnosis was provided. Does this refer to newly diagnosed diabetic participants? Please clarify this issue.
- Methods: as abovementioned, please specify if income refers to individual or household income.
- Methods: the authors also refer to a Socio-economic status (SES) score (table 3). However, results across SES categories are not fully consistent with those by income or education. What is the correlation between these different measures? How does SES differ from income or education? Please clarify this issue.
- Methods: how similar or different are the traditional Mediterranean score and the Italian Mediterranean index?

	- Results: in tables 1 & 3, unlike for obesity, hypertension prevalence did not significantly differ across income, SES and education categories. How do the authors explain this? - Results: in table 2, it is interesting to note that energy intake did not really differ across income categories, which is not fully consistent with the notion that lower SES groups will consume more energy dense diets. Do the authors have any data on sugars or refined grains? - Results: it is not really clear if the data on the three dietary patterns, derived by principal components analysis, provides and additional information above and beyond the two Mediterranean scores. Please clarify this issue. - Results: table 3 seems redundant and could be removed and discussed in the text. - Results: in my view, table 5 provides the most interesting findings of this study. In fact, this table suggests that income is associated with dietary patterns above and beyond education. I wonder if the reverse is true, i.e. does education relate to dietary patterns independent of income? - Results: adjusting for sex and age is fine. However, I wonder if results would look any different by gender or across age groups. This information would further enrich the present manuscript. - Discussion: on page 12, I would disagree with the statement that lack of knowledge about the healthy benefits of MD does not play any role. In fact, the result that education was associated with a greater adherence to Mediterranean diet, as observed in this study, seems to confute the previous statement. - Discussion: we are aware that Molise is in the central-southern part of Italy. However, this region is predominantly rural, with likely higher intakes of meat and dairy products as compared to the original Mediterranean dietary patterns, as observed by Ancel Keys. Hence, I wonder whether this aspect may have any role in the observed associations in the present study. The authors should discuss this issue. - Conclusion: the concluding statement that these "data clearly indicate that eating by the Mediterranean way is also, if not mainly, a matter of healthy food accessibility in terms of economic costs" is likely to be true. However, this statement cannot really be proved by the present study due to its observational, cross-sectional nature. Actually, this study suggests consistent associations of income and education with dietary patterns. I would therefore suggest moderating the assertive tone of the concluding remarks.

THE STUDY	Is the research question clearly defined? No, the research question is not clearly defined. For instance, the objective stated in the abstract does not include obesity although the title of the manuscript title and the conclusion of the abstract is about the relationship between income and education, adherence to Mediterranean diet patterns and obesity. It seems that the authors have two distinct objectives. See at the end of the introduction: - "The aim of the present study was to assess possible differences in eating patterns among adult Italians with different socio-economic status, with particular focus on

	low-income"
	- Our study aimed also at evaluating a potential relationship between low-income and obesity or overweight.
	Is the overall study design appropriate and adequate to answer the research question? Are the statistical methods appropriate?
	It is difficult to answer because the research question is not clear. However, the study would have been original and new if the authors have used models allowing them to combine and to compare the impacts of socio-economic status (education, income) on diet quality and then on obesity in a unique causal model. Instead of that, they have addressed the relationships in parallel.
	Thus, the results are not really attractive because it is known from a long time that i) people with a low socioeconomic position have less healthy diets than those in higher socioeconomic positions; and that ii) obesity prevalence is higher in low-income less-educated populations…
	Is the main outcome measure clear?
	No, the main outcome measure is not clear. Is it obesity? Is it having a poor diet? Is it diet cost (in that case, it is problematic because it was not measured at all)?
	Are the references up to date and relevant? (If not, please provide details of significant omissions below.)
	For instance, the authors cite ref 6 for stating that "Increasing prices of many of the basic food items of MD seem to have led people to give up this eating pattern in favour of less expensive products which allow to save money but are definitively unhealthy" but food price variations were not assessed in ref (6).
	For the relationship between obesity and socioeconomic status, the authors cite one study from France. I wondered why this specific study was cited, although comprehensive reviews have been published on that topic, in particular McLaren L. Socioeconomic status and obesity. Epidemiol Rev 2007;29:29-48.
	Page 12, the authors state " This study contributes to provide further evidence to the assumption that dietary habits are strongly influenced by socio-economic status, in particular by income which appears to play an important role in determining people's eating choices." They could have cited "Darmon N, Ferguson EL, Briend A. A cost constraint alone has adverse effects on food selection and nutrient density: an analysis of human diets by linear programming. J Nutr 2002;132:3764-71." which is the only demonstration, based on simulation and modeling, of the causality between cost constraints and unhealthy food choices.
RESULTS & CONCLUSIONS	Everywhere in the paper (see for instance: the conclusion of the abstract; lines 49-54 page 4 in the introduction; lines17-21 and 20-33 p11 in the discussion; lines 22-24 page 14 in the conclusion, etc…), they insist on the "increasing price of the basic Mediterranean foods" and, more generally on "matter of healthy food accessibility in terms of economic costs" to explain their results, but nothing in their study allows them to draw such a conclusion because first, the study was transversal, and second, they did not collect food prices. It seems that they make a confusion, between having a low income and facing high food prices.

		Most of the conclusions are overstated and cannot be directly drawn from the results of the study. For instance, page 12 " We started our study wondering about what makes so hard for people to choose healthy food instead of bad products, putting at risk their own health. We excluded it could be just a matter of personal choice or taste, neither an issue related to the lack of knowledge about the healthy benefits of MD worldwide recognized and also well disseminated in the lay press". I see nothing in their results that can authorize them to conclude that. I see only one sentence reflecting what has been done and not more. It is page 12, lines 27-30: "Our study highlights the strong linkage among low income, poor adherence to MD and consequent obesity". And yet, I would remove "consequent" because no causal relationship can be demonstrated, given the design and the statistical analysis performed (logistic models, at best).
REPORTING & ETHICS		
GENERAL COMMENTS		

VERSION 1 – AUTHOR RESPONSE

Reviewer: Michel de lorgeril, MD
School of medidine of the Grenoble University
France

Regarding the evidence of health benefits of the Mediterranean diet, it should be fair to cite the unique randomised trial having tested it, for instance the final report of the Lyon Diet Heart Study (in Circulation in 1999) unambiguously showing strong protection against CVD complications. Results of the trial were then confirmed by several observational studies; important to underline however that randomised trials represent a better evidence than any meta-analysis of observational studies. In any case, both the trial and observational studies showed very similar benefits (also for cancers) which is rarely the case in nutrition epidemiology; see for instance the "antyioxidant story" ...

We followed the referee's suggestion and added references 2 and 3 in the introduction.

Regarding the real cost of adhering to the Mediterranean diet model, it should be important to also discuss (in addition to the prices of the foods) the health economic advantages of reducing the rate of CVD complications. It would be fair for instance to cite and discuss the following article (even if it was in the context of secondary prevention): "A mediterranean diet is cost-effective in patients with previous myocardial infarction." By Dalziel K, Segal L, de Lorgeril M. In J Nutr. 2006 Jul;136(7):1879-85.

We thank for the suggestion. The article has been cited and discussed in the discussion paragraph.

This is an excellent article raising the major issue of the relations between diet quality and socio-education status. I encourage the publication.
Minor points:
- about education level, why only two categories? It seems restrictive ...

We performed new analyses by using a 3 level education variable and reported the new results both in the text and in table 4 . Indeed the results appear still consistent.

- use of three different parameters to evaluate the Mediterranean diet pattern: this is obviously a strength of that study! However, it would be interesting for many readers to explain why each of these parameters was not satisfying and why the three are needed; in other words, please, explain the weakness of each parameter!

We have discussed the issue in the "Strength of the study" of the Discussion section.

- there are more women in the low income category. It is not surprising but most middel-aged women are married and their own income (additional salary) could not be representative of their real income? Is the marital status important to include as covariable? Did you do it?

We agree that marital status may be a potential confounding factor, thus we included it as a covariable in all multivariable analyses.

- Very low current smoking rate in all categories: reporting bias?

Actually, the rates are similar to those provided by the Italian cardiovascular observatory 1998-2002 (http://www.cuore.iss.it/fattori/fumo.asp) which reports current smoking rate as 28%.

- Very high hypertension rate (>50%) in all categories: true or bias? Explanations?

We apologize for this mistake, correct definition and values are now reported in methods and table 1.

- Alcohol intake quite high in all categories: true?

We checked the data and the values reported are correct.

- No data on wine intake?

We have now reported wine intake in table 2.

- No data on oils other than olive oil?

We have now reported data in table 2.

- No data on fish species? Fatty fish?
- No data on other sea foods very characteristic of the Mediterraneans? Octopus?

We have now reported data on crustaceous and mollusk intake in table 2.

Reviewer: Saverio Stranges, MD, PhD
Associate Professor of Cardiovascular Epidemiology
Statistics & Epidemiology

Division of Health Sciences
University of Warwick Medical School
I do not have any competing interests with the present manuscript.

The English needs some revision. Some sentences were unclear, with a few grammatical errors throughout the manuscript.

This is an interesting manuscript from the Moli-sani project, a large, well-conducted epidemiological study from Southern Italy. The study addresses an important issue reflecting a rapidly evolving scenario. Specifically, the authors examined cross-sectional associations of socio-economic status (i.e., income and education) with adherence to a healthy Mediterranean dietary pattern as well as with obesity prevalence.

This is an important public health issue given the on-going epidemiological transition in several countries of the Mediterranean region, namely southern Italy and Greece, which were traditionally associated with healthier lifestyles and dietary patterns than those observed in Northern European and American populations. However, recent evidence suggests that these countries have been gradually abandoning their original dietary patterns, at least in disadvantaged population subgroups, as a result of major societal changes and increasing financial constraints over the last few decades. Hence, there has been an escalating prevalence of major CVD risk factors in these populations, primarily obesity and hypertension, as well as suggestive evidence of leveling off or potential reversing of CVD mortality trends in these settings.

As expected, the authors found that higher income and education were associated with a greater adherence to a Mediterranean dietary pattern and with a lower prevalence of obesity. These results reiterate the central role of socio-economic status in the adoption of healthy lifestyle and eating behaviours, which will eventually translate in lower morbidity and mortality from major chronic, diet-related diseases. These findings are of public health importance and further emphasise the urgent need for preventive policy measures, which have the potential to tackle the escalating prevalence of major CVD risk factors in these populations.

Major strengths of the present manuscript are the large, communitywide sample, as well as state-of-art measures for several of the variables included in the analysis.

However, there are some specific concerns and questions with the present study, which should be addressed to further improve the overall merit and clarity of this manuscript.

Specifically:

- Abstract: I would suggest rephrasing the objectives of this study with a sentence like "to examine cross-sectional associations of socio-economic status (i.e., income and education) with adherence to a Mediterranean dietary pattern and obesity prevalence". This would be in line with the title.

We rephrased the sentence as suggested.

- Abstract: please specify that income refers to individual rather than household income.

Income refers to household net income. We specified it in the abstract and in the Methods paragraph.

- Abstract: it seems that income is associated with dietary patterns in both higher and lower education

subgroups (table 5). This is an important observation, which should be highlighted in the abstract.

A sentence has been added in the Abstract.

- Abstract: I would suggest removing the last sentence of conclusions "The increasing prices of the basic Mediterranean food items seem to represent a real obstacle to healthy diet driving people to choose alternative ways of eating usually inspired by the need to save money in everyday life." This is likely to be true. However, this speculative statement should be placed in the discussion only, because there are also other likely explanations for this changing scenario. In addition, given its observational, cross-sectional design, the present study cannot definitely prove this notion.

We agree with the reviewer and removed the sentence.

- Introduction: the authors should also cite some recent evidence from other Italian studies, supporting the notion the southern populations have unfortunately lost their advantageous "Mediterranean" cardio-metabolic risk profiles (see for example: Laccetti T et al. Public Health Nutr. 2012;30:1-11).

This reference has now been cited in the Introduction.

- Methods: despite the cross-sectional nature of the present analysis, we are aware that there is an on-going prospective follow-up in the Moli-sani project. The authors should clarify this aspect to justify the use of the terminology "cohort study" when referring to this project.

A sentence has been added in the Methods.

- Methods: the large number of missing data regarding income is obviously a potential limitation of this analysis, and may raise the potential of selection bias. The authors have fairly acknowledged this issue in their discussion.

We have stressed this potential bias in the paragraph on the limitation of the study.

- Methods: participants with prevalent CVD (5.7%) were excluded. Can the authors clarify what type of CVD fall in this category? Also, it seems that diabetics (6%) were excluded. However, in the paragraph on data collection, definition of diabetes diagnosis was provided. Does this refer to newly diagnosed diabetic participants? Please clarify this issue.

We have clarified the definition of CVD in the Methods. Our sample did not include subjects with diabetes, the definition was a mistake. We have deleted it from the text.

- Methods: as abovementioned, please specify if income refers to individual or household income.

We specified that income referred to household net income.

- Methods: the authors also refer to a Socio-economic status (SES) score (table 3). However, results across SES categories are not fully consistent with those by income or education. What is the

correlation between these different measures? How does SES differ from income or education? Please clarify this issue.

Socio-economic status (SES) was expressed as a score based on 5 variables: dwelling ownership and ratio between the number of living-in family members and number of rooms (People Room Density), both currently and at childhood - and availability of hot water at home at childhood and it does not include income or education. The correlation among SES, income and education has been reported in the Results (paragraph 1). The low correlation between SES and income could be due to the frequent dwelling ownership at childhood in rural Southern Italy communities.

- Methods: how similar or different are the traditional Mediterranean score and the Italian Mediterranean index?

We added in the Method section. More information on how the two indexes were calculated.

- Results: in tables 1 & 3, unlike for obesity, hypertension prevalence did not significantly differ across income, SES and education categories. How do the authors explain this?

We repeated the analysis with the use of the correct definition of hypertension. We found difference in row prevalence of hypertension across strata of income, SES and education that were significant in univariable analysis, but disappeared after adjustment for age, sex, energy intake and marital status. This is in contrast with the value of both systolic and diastolic blood pressure that remained significantly lower in the high income (SES and education) category after multivariable analysis. Probably age might be a stronger confounder for hypertension; in alternative, the definition of hypertension also based on current drug therapy could account for the difference. Indeed the access to diagnosis and therapy in the Italian health system should be less dependent on income.

- Results: in table 2, it is interesting to note that energy intake did not really differ across income categories, which is not fully consistent with the notion that lower SES groups will consume more energy dense diets. Do the authors have any data on sugars or refined grains?

Sugar and refined grains consumption are now reported in table 2.

Results: it is not really clear if the data on the three dietary patterns, derived by principal components analysis, provides and additional information above and beyond the two Mediterranean scores. Please clarify this issue.

We used "a priori" and "a posteriori" dietary analysis to overcome the limitations each of these approaches individually present. Indeed, "a priori" scores only reflect some aspects of diet and do not account for correlations between score components. Instead, "a posteriori" approaches have the weakness of low reproducibility: different populations may have different non-predefined dietary patterns. Therefore, the use of an index based on the foods actually available to Italians and traditional Italian cooking styles should improve the ability of the index to classify the Italian cohort. The two approaches gave similar results, reinforcing the values of our observation. This issue has been discussed in the strength of the study paragraph, also following the suggestion of reviewer 1. We added this comments in the Strengths of the study paragraph.

- Results: table 3 seems redundant and could be removed and discussed in the text.

Previous Table 3 has been removed and results on the association between SES and dietary score have been added in the present table 4 and discussed in the text.

- Results: in my view, table 5 provides the most interesting findings of this study. In fact, this table suggests that income is associated with dietary patterns above and beyond education. I wonder if the reverse is true, i.e. does education relate to dietary patterns independent of income?

We thank the referee for his important suggestion. Education relates indeed to dietary independent of income. We divided the income levels into two categories (low and low-medium vs high and high-medium) and evaluate the association between education and dietary patterns according to these two income categories. We found that in the lowest income group, education was positively associated with dietary patterns in the multivariable model (p = 0.032 for MDS and p = 0.0025 for IMI). The same positive trend was observed in the highest income group (p = 0.0067 for MDS and p = 0.0010 for IMI). Thus we can safely conclude that education relates to dietary pattern independently from income. These results have been now discussed in the Results paragraph.

- Results: adjusting for sex and age is fine. However, I wonder if results would look any different by gender or across age groups. This information would further enrich the present manuscript.

We thank the referee for his suggestion. We added analyses stratified by age and sex in the Results section.

- Discussion: on page 12, I would disagree with the statement that lack of knowledge about the healthy benefits of MD does not play any role. In fact, the result that education was associated with a greater adherence to Mediterranean diet, as observed in this study, seems to confute the previous statement.

We agree and deleted our misleading sentence.

- Discussion: we are aware that Molise is in the central-southern part of Italy. However, this region is predominantly rural, with likely higher intakes of meat and dairy products as compared to the original Mediterranean dietary patterns, as observed by Ancel Keys. Hence, I wonder whether this aspect may have any role in the observed associations in the present study. The authors should discuss this issue.

Molise region is a predominant rural area, however 60% of our population is resident in the two main cities of Campobasso and Termoli. Moreover, the mean consumption of meat and dairy products of our population is comparable with the national mean, as reported by INRAN (www.INRAN.it).

- Conclusion: the concluding statement that these "data clearly indicate that eating by the Mediterranean way is also, if not mainly, a matter of healthy food accessibility in terms of economic costs" is likely to be true. However, this statement cannot really be proved by the present study due to its observational, cross-sectional nature. Actually, this study suggests consistent associations of income and education with dietary patterns. I would therefore suggest moderating the assertive tone of the concluding remarks.

We agree with the reviewer and rephrased the sentence accordingly.

Reviewer: Nicole Darmon, INRA, France. I have no competing interest in link with this study.

Is the research question clearly defined?
No, the research question is not clearly defined.
For instance, the objective stated in the abstract does not include obesity although the title of the manuscript title and the conclusion of the abstract is about the relationship between income and education, adherence to Mediterranean diet patterns and obesity.
It seems that the authors have two distinct objectives. See at the end of the introduction:
- "The aim of the present study was to assess possible differences in eating patterns among adult Italians with different socio-economic status, with particular focus on low-income"
- Our study aimed also at evaluating a potential relationship between low-income and obesity or overweight.

We have rephrased and clearly stated the objectives of our study, also according to reviewer 2, both in the abstract and the introduction.

Is the overall study design appropriate and adequate to answer the research question? Are the statistical methods appropriate?
It is difficult to answer because the research question is not clear. However, the study would have been original and new if the authors have used models allowing them to combine and to compare the impacts of socio-economic status (education, income) on diet quality and then on obesity in a unique causal model. Instead of that, they have addressed the relationships in parallel.

We thank the referee for this important observation. As she suggested, we performed further analysis allowing to combine the impacts of education and income on diet quality and then on obesity in a unique causal model. Firstly, we confirmed the strong association of income with obesity: odds ratio of having obesity decreased according to income and education level (data are now shown in the Income, Mediterranean diet and obesity paragraph of the Results section). These odds ratio remained unchanged when diet quality was included in the model. Additionally, the association of income or education with obesity was similar according to diet quality (data added). According to these results, we are not allowed to state that the changes in obesity rates observed in the different income and education categories are mediated by diet quality. In other words, the lowest rate of obesity observed in the uppermost income categories could not be fully ascribed to the adherence to healthier diets. These data have now been included in the Results and Discussion sections.

Thus, the results are not really attractive because it is known from a long time that i) people with a low socioeconomic position have less healthy diets than those in higher socioeconomic positions; and that ii) obesity prevalence is higher in low-income less-educated populations…

We have mentioned results from previous literature in our manuscript; however we believe our study has some original features that is the use of different dietary scores (derived from both a priori and a posteriori approaches) and the demonstration we provided that income and education still play a role in influencing dietary choices in a population living in an environment homogeneous both for genetic and lifestyles.
Furthermore, diet quality showed a continued improvement across a relatively small range of economic strata and the association between education and dietary habits appears to be independent

from income (as also underlined by rev.2). In addition, as previously stated, we found that the lowest rate of obesity observed in the uppermost income or education categories could not be ascribed to the adherence to healthier diets.

Is the main outcome measure clear?
No, the main outcome measure is not clear. Is it obesity? Is it having a poor diet? Is it diet cost (in that case, it is problematic because it was not measured at all)?

The main outcome was to demonstrate the association between income and dietary score, evaluated by the "a priori" and the "a posteriori" approaches. As secondary aim, we evaluated the association between income and obesity. Moreover, as suggested by this reviewer, we have now evaluated the association of income with dietary score and obesity in a combined model. We have clarified the issue in the text.

Are the references up to date and relevant? (If not, please provide details of significant omissions below.)
For instance, the authors cite ref 6 for stating that "Increasing prices of many of the basic food items of MD seem to have led people to give up this eating pattern in favour of less expensive products which allow to save money but are definitively unhealthy" but food price variations were not assessed in ref (6).

We have clarified this sentence according to the reviewer's suggestion.

For the relationship between obesity and socioeconomic status, the authors cite one study from France. I wondered why this specific study was cited, although comprehensive reviews have been published on that topic, in particular McLaren L. Socioeconomic status and obesity. Epidemiol Rev 2007;29:29-48.

We agree and included the indicated reference.

Page 12, the authors state " This study contributes to provide further evidence to the assumption that dietary habits are strongly influenced by socio-economic status, in particular by income which appears to play an important role in determining people's eating choices." They could have cited "Darmon N, Ferguson EL, Briend A. A cost constraint alone has adverse effects on food selection and nutrient density: an analysis of human diets by linear programming. J Nutr 2002;132:3764-71." which is the only demonstration, based on simulation and modeling, of the causality between cost constraints and unhealthy food choices.

We agree and included the indicated reference.

Everywhere in the paper (see for instance: the conclusion of the abstract; lines 49-54 page 4 in the introduction; lines17-21 and 20-33 p11 in the discussion; lines 22-24 page 14 in the conclusion, etc…), they insist on the "increasing price of the basic Mediterranean foods" and, more generally on "matter of healthy food accessibility in terms of economic costs" to explain their results, but nothing in their study allows them to draw such a conclusion because first, the study was transversal, and second, they did not collect food prices. It seems that they make a confusion, between having a low income and facing high food prices.

We rephrased the manuscript according to the reviewer's suggestions.

Most of the conclusions are overstated and cannot be directly drawn from the results of the study. For instance, page 12 " We started our study wondering about what makes so hard for people to choose healthy food instead of bad products, putting at risk their own health. We excluded it could be just a matter of personal choice or taste, neither an issue related to the lack of knowledge about the healthy benefits of MD worldwide recognized and also well disseminated in the lay press". I see nothing in their results that can authorize them to conclude that.

We agree and deleted the statement.

I see only one sentence reflecting what has been done and not more. It is page 12, lines 27-30: "Our study highlights the strong linkage among low income, poor adherence to MD and consequent obesity". And yet, I would remove "consequent" because no causal relationship can be demonstrated, given the design and the statistical analysis performed (logistic models, at best).

We eliminated the word "consequent", as suggested.

VERSION 2 – REVIEW

REVIEWER	Michel de Lorgeril School of Medicine University of Grenoble No competing interest
REVIEW RETURNED	17-Sep-2012

- The reviewer completed the checklist but made no further comments.