Author's response to reviews

Title: Association of socioeconomic status with overall overweight and central obesity in men and women: the French Nutrition and Health Survey 2006

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Author's response to reviews: see over
Dear Editor,

Please find enclosed our revised manuscript entitled “Association of socioeconomic status with overall overweight and central obesity in men and women: the French Nutrition and Health Survey 2006”.

We are grateful to the referees for their constructive comments and you will find enclosed a cover letter giving a point-by-point response to the comments.

We therefore hope that our revised manuscript will comply with your expectancy.

Sincerely Yours,

Dr. Michel Vernay on behalf the authors
Editor comment:
(1) Document, within the methods section of your manuscript, the name of the ethics committee which approved your study.

One sentence has been added (p5): “The survey protocol received the approval of the Ethical Committee (Hôpital Cochin n°2264), the Consultative Committee on Information Treatment of the Ministry of Research and the French Data Protection Authority (Commission nationale de l’Informatique et des Libertés, CNIL, authorization n°905481).”
Reviewer's report:

Manuscript “Association of socioeconomic status with overall overweight and central obesity in men and women: the French Nutrition and Health Survey 2006” evaluates social inequalities in the very important risk factor for various health outcomes. This is an important topic which is studied in representative sample of large European population. The authors present interesting findings in well written paper. I have few comments that might be of interest.

Major Compulsory Revisions:

Response rate may be quite big problem in this study. It seems that 46.3% response rate for anthropometric measures is quite low. After excluding people with incomplete data the final dataset has 2204 subjects which is 42% of original sample. I am not sure how representative this sample is of the whole French population and whether authors can make any conclusions about levels of obesity in French population and comparisons with other populations. I am not sure whether the association between SES and obesity can be assessed in such sample.

Indeed, the representative nature of the sample is a key point of the study. Weighting methods included calculation of probabilities of sample inclusion taking into account multilevel sampling design (Svyset procedure from Stata®) and calibration according to national census data on age, educational diploma and whether or not the household included at least one child. The weighted sample was therefore representative of the adult population for at least these characteristics. In order to clarify the text, a sentence has been added to the statistical section (page 6): “For each gender, calibration was calculated according to the national census data on age, education diploma and whether the household included or did not include at least one child.” Since such a detail was included, we also added this sentence: “The final weighting also included the period of measurements.”

Table 1 has been modified in order to present social and demographic characteristics of the sample before and after weighting procedures. Two sentences have been added in the results section “Characteristics of the subjects” (p7): “Young adults and subjects with a low level of education were poorly represented in the initial sample (Table 1). Other sociodemographic characteristics were scarcely modified by the weighting procedure.” Concerning the small sample size and participation bias, we have also modified the discussion section (p12), by adding “The rather small sample size, particularly for men, may have limited the power of the study. Moreover, the initial sample was also characterized by underrepresentation of young adults and subjects with a lower education level, suggesting a participation bias [10].” with the reference of the published article when these aspects were discussed.
Concerning a participation bias, we have also added one sentence in the discussion section (p12), explaining why we used the calibration procedure in order to limit the impact of participation bias: “The participation bias was taken into account by a calibration procedure according to national census data on age, education level and the presence, or not, of at least one child in the household.”

Another sentence was added in the same section regarding characteristics of the weighting sample compared to national census data: “Moreover, the proportion of subjects who did not take a holiday trip during the past twelve months and distribution according to occupation were similar in the weighted sample and in the national census data [12].”

Results: authors show results for overweight and central obesity but they do not show any results for obesity. This might be also included in the manuscript.

We have modified the results section (p8), adding two sentences on results regarding overall obesity in men – “In multivariate analysis, overall obesity was not associated with SES characteristics (data not shown).” and in women “In multivariate analysis, similar associations were obtained for overall obesity (data not shown).”

Only very few results are shown as significant - this may be partly due small sample size (and power) of the study. Some of the OR are quite large and the effects might be important however authors do not have power to have these findings statistically significant. One way how to improve the power is to use interaction terms in the regression modelling rather than splitting sample by sex.

As you mention in your first comment, the rather small sample size could be a crucial point of the survey. However, two main reasons led us to analyze men and women separately. (1) We tested interactions between gender and explanatory variables. Regarding overweight and obesity, there were significant interactions between sex and birthplace, occupation, education level, holidays and smoking habits. Concerning central obesity, there were significant interactions between sex and area of residence and smoking habits. (2) We hypothesized (as commonly tested in the literature) that SES effects on overweight and obesity might differ according to sex, as mentioned in the methods section (p7).

In order to clarify this point, we modified the methods section (p6-7): “Interactions between gender and several factors (birthplace, occupation, education, holidays and smoking habits for overweight and obesity, and area of residence and smoking habits for central obesity) suggested potential differing associations according to gender.”

Authors need more systematic approach to be able to discuss effects of main socioeconomic characteristics on obesity (education-related, work-related and wealth-related). (see comment about stepwise regression below in Minor revisions section).

We hope that all modifications regarding statistical methods (weighting and calibration procedures, small sample size and participation bias, interactions between gender and
explanative variables, logistic regression) have contributed to improving the manuscript and clarifying the approach we used.

Minor essential revisions:
The authors need to use consistent terminology throughout the paper. They use terms “central adiposity”, “central obesity”, “central overweight” and I would thing that one term used in the whole paper would make some parts of the text clearer.

The manuscript has been modified as necessary.

Methods: could the authors describe how the methods used were standardised?

As previously mentioned in the anthropometric section (p5), all anthropometric measurements were carried out according to WHO procedures and using the same devices. All interviewers (physicians, nurses and dieticians) were specifically trained for data collection. We have added the word “specifically” in the relevant sentence in the anthropometric section (p5).

It has been shown in literature that measurements in clinical setting differ from measurements done in home settings. Authors stated in their methods that anthropometric measures were measured at home or in health examination centre. Could they describe what was the proportion of measurements done at home? Did they test whether these measurements systematically differ between home and clinic?

The results section (p8) now provides information on measurement locations: “Body height and weight were measured at a health examination centre for 50.6% of participants and at home for 49.4%”.

The mean differences between measurements at home and at health examination centres have been tested. A sentence has been added in the section on characteristics of the subjects (p8): “(...) mean anthropometric measurements were not statistically different according to the place where measurements were taken.”

Related to this: were socioeconomic and behavioral data also partly collected at home and partly at the clinic?

Socioeconomic and behavioural data were entirely collected at home. The information has been added in the section on socioeconomic and behavioural data (p5).

The data are part of Nutrition Survey but no nutritional data are used among health behaviours? Why?

Several factors led us to exclude dietary data. (1) Part of the food consumption analysis had already been published in the British Journal of Nutrition (reference 10 has been added in the manuscript). (2) Analyses of factors associated with food consumption constitute a distinct objective. As potential “intermediate” factors explaining the
association between SES and obesity, the inclusion of dietary data in statistical analyses may have masked the observed associations, particularly in a cross-sectional survey. Specific analyses will eventually be carried out.

Statistical methods: I am not in favour of stepwise regression - the authors let computer to decide whether to keep variables in the model rather than to keep key variables in the model. This procedure results in rather difficult gender comparison of multiple regression models. None of the key variables is shown for both men and women and comparison is rather difficult.

We carried out manual logistic regression in order to identify factors to be retained in the final model. Finally, as mentioned above, interactions between SES and sex were observed. One sentence has been added in the statistical analysis section (p6): "Factors associated with inclusion at alpha ≥ 5% were retained in the final model, except when their exclusion led to an OR variation > 10%”. The adjective “manual” has been added to the previous sentence and the term “stepwise” has been deleted.

Results (page 7) - authors show p values < 0.001 but it is not complete clear what is the origin of these p-values. Do they come from LR tests?

Yes they came from LR tests. The words “and p-values (p)” have been added to the sentence detailing calculation of OR and CI in statistical analyses (p6).

Did the authors test colinearity between socioeconomic characteristics (such as education and employment)? What was the correlation coefficient, for example?

Indeed, there was an association between education and occupational distribution. The ENNS survey constituted an opportunity to analyze SES variables together. Indeed, analyzing such factors together is of interest, since they underline different mechanisms, as presented in the discussion, especially in terms of nutritional behaviour and status. In order to clarify this point in the manuscript, we have added a sentence in the discussion section (p10-11) along with a reference: “Although, occupation, education level and income are not completely independent, it is of interest to analyze these three SES dimensions together [29].”

Discussion: are there other publications in other European populations> It seems that the authors use rather limited number of results for comparison of own results with previous literature.

Sobal and Stunkard (1989) published a first review on the association between obesity and socioeconomic factors, mainly based on cross-sectional surveys and including European surveys. Ball and Crawford (2005) carried out a similar study using mainly longitudinal data. In order to render the reference list more concise, we limited the number of listed surveys. References 24, 25, 26, 27, 28 and 29 were from European surveys.
Limitations of the study (such as already mentioned low response rate or sample size) are briefly mentioned but these should be discussed in larger extent, particularly how they could affect the main findings of this paper.

In addition to clarifying the response rate (see first comment), two sentences have been added to the discussion (p12): “The rather small sample size, particularly for men, may have limited the power of the study. Moreover, the initial sample was also characterized by underrepresentation of young adults and subjects with a lower education level, suggesting a participation bias [10]”. The two new sentences are as follows: “The participation bias was taken into account by a calibration procedure according to national census data on age, education level and the presence, or not, of at least one child in the household. Moreover, the proportion of subjects who did not take a holiday trip during the past twelve months and distribution according to occupation were similar in the weighted sample and in the national census data [12]”.

Discretionary revisions: Authors put smoking habits among SES measures in the methods. I would probably grouped them with alcohol into health behaviours.

The sentence “Face-to-face interviews also included questions on smoking habits” has been added following the explanation of how alcohol consumption data were collected (p5, line 8 of “Socioeconomic and behavioural data”).

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests: I declare that I have no competing interests
Major revision

p 3 Background; p 9 Discussion; Conclusion: “The prevalence of overweight and obesity is lower in France than in most industrialized countries.”

Prevalence of obesity in France is certainly lower than in the UK and eastern Europe countries but not lower than in neighbour countries in western Europe such as the Netherland, Switzerland, Sweden, Norway Italy, Spain… There is no reason to make a special case for France.

We agree that those sentences were not entirely clear. We have modified the text as follows:

- **Background section (p3)**: the sentence has been modified: “In France, as in neighbouring western Europe countries, the prevalence of obesity among adults is considered to be lower than that reported in the USA, Canada, the UK and eastern Europe [1,2]”.

- **Discussion section (p10)**, the sentence has been modified: “ENNS data confirm that France, like several neighbouring western European countries, is less strongly affected by overweight and obesity than the UK and eastern Europe countries [24]”.

- **Conclusion section (p13)**, the sentence has been modified: “In France, the prevalence of overweight and obesity is low compared to that in the USA, Canada, the UK and Eastern Europe countries”.

Conclusion 15-9: References needs to be provided for these two affirmations or the sentences should be deleted.

References concerning socio-environmental factors associated with obesity have been added to the discussion (p13). In order to be more coherent, “In the USA” has been added at the beginning of the sentence; in the sentence which follows, “are” has been replaced by “seem”.

p 6 : Methods: Principles of the methods for weighing the data needs to be provided as well as the characteristics of the sample according to the weighting variables before, after weighting, and similarly in the reference french population.
Weighting methods included calculation of probabilities of sample inclusion taking into account multilevel sampling design (Svyset procedure of Stata®) and calibration according to the national census data on age, education diploma and whether or not the household included at least one child. The weighted sample was therefore representative of the adult population for at least these three characteristics. In order to clarify the manuscript, one sentence has been added in the statistical section (page 6): “For each gender, calibration was calculated according to the national census data on age, education diploma and whether the household included or did not include at least one child.”. Since such an explanation was included, we have also added this sentence: “The final weighting also included the period of measurement ”. Table 1 has been modified in order to present social and demographic characteristics of the sample before and after weighting procedures. Two sentences have been added in the results – characteristics of the subjects (p7): “Young adults and subjects with a low level of education were poorly represented in the initial sample (Table 1). Other sociodemographic characteristics were scarcely modified by the weighting procedure.” Concerning a participation bias, we have added one sentence in the discussion (p12), explaining why we used the calibration procedure to limit the impact of participation bias: “The participation bias was taken into account by a calibration procedure according to national census data on age, education level and the presence, or not, of at least one child in the household.” Another sentence was added in the same section regarding characteristics of the weighting sample compared to national census data: “Moreover, the proportion of subjects who did not take a holiday trip during the past twelve months and distribution according to occupation were similar in the weighted sample and in the national census data [12].”

Minor revision

p 6: Characteristics of the subjects: Can the author add some information about the comparison of subjects who did or did not participate into the study (at minimum distribution according to area of residence) and on reasons for refusals?

As mentioned in the sample design (p4), sample selection was stratified on thirty-two strata based on eight large regions and by the degree of urbanization (four groups from “rural” to “towns of more than 100,000 inhabitants”). We have added two sentences in the results (p7) concerning the participation rate according to area of residence: “Compared to other degrees of urbanization, the participation rate was weaker in the Paris area (12.6% of participants were from the Paris area, even though Paris area inhabitants represent 17% of the French adult population). Lack of time available for participating in data collection, along with only minor interest in nutrition, were the main reasons cited for refusal.”

p 7-8: Results: Age is accounted for in 2 broad categories in the multivariate analysis. Residual confounding with age is possible. For example, it may explained the high OR for obesity in retired women persisting after age adjustment.
After age adjustment, occupation was retained in multiple logistic regression models only in men. In retired men, there was a significantly increased risk of overweight and obesity (but not of central obesity). Two sentences have been added to the discussion (p12): “Because of age categories, a residual confounding effect of age may remain when analyzing a potential association with occupation. Indeed, use of continuous age rather than age categories led to a non-significant OR in retired men, while it remained significant in the category of self-employed and farmers”

P 9, l 16: the relationship between little job control and little leisure-time physical activity is not obvious to me.

In order to clarify the relationship between low job control and little leisure-time physical activity, we modified the sentence in the discussion (p10): “Occupation is considered to reflect job control, and a low employment position is associated with less time available for leisure and physical activities [25].”

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

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

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