Author's response to reviews

Title: The positive association between number of children and obesity in Iranian women and men: Results from the National Health Survey

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Version: 5 Date: 16 March 2008

Author's response to reviews: see over
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Version: 3 Date: 15 march 2008

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Reviewer's report
Title: The positive association between number of children and obesity in Iranian women and men: Results from the National Health Survey
Version: 2 Date: 11 February 2008
Reviewer: Rebecca Hardy

Reviewer's report:

- Major Compulsory Revisions

1. The unadjusted association between number of children and obesity should be presented as well as the adjusted one, so that the influence of the potential confounders included in the analysis can be observed.

   * The reviewer is correct and we have added these sentences to the second paragraph of the Results:

   We started by fitting a model containing sex and number of children to observe the influence of the potential confounders included in the analysis. In the GEE analysis, the odds of obesity increased with increasing number of children: unadjusted obesity odds ratios were 1.29(95% CI: 1.21-1.39) and 1.10(95% CI: 1.001-1.20) for women and men, respectively. The OR of 1.29 for women was significantly different from the OR of 1.10 for men because the interaction between the number of children and sex was statistically significant (p=0.006).

   * We have also added these sentences to the first paragraph of the Discussion:

   In first model (without confounders), unadjusted obesity odds ratios were 1.29(95% CI: 1.21-1.39) and 1.10(95% CI: 1.001-1.20) for women and men, respectively. Unadjusted OR of 1.29 for women was significantly different from the Unadjusted OR of 1.10 for men.

2. There needs to be more discussion on the possible mechanisms relating to the association in women as well as men. The fact that similar associations were seen in both genders suggests a lifestyle, rather than a biologic, impact of having children in women. The extent of change in the association before and after adjustment for the confounding variables would be useful in informing this discussion.

   * The reviewer is correct and we have added these sentences (second, third and fourth paragraphs) to the Discussion:

   Comparing two models the influence of the potential confounders included in analysis can be observed. Unadjusted OR of 1.29 and 1.10 are changed to OR of 1.16 and 1.18 for women and
men, respectively. After controlling for potential confounders, the similar associations were seen in both sexes suggest a lifestyle, rather than a biologic, impact of having children in women. Our findings were consistent with the conclusions of Lawlor et al. [35] and Hardy et al. [36]. Lawlor et al. [35] concluded that "Lifestyle risk factors associated with child-rearing lead to obesity and result in increased CHD in both sexes; biological responses of pregnancy may have additional adverse effects in women". Hardy et al. [36] showed that "Any association between number of children and CHD risk factors is a result of lifestyle and behaviors associated with family life rather than being as result of the biological impact of pregnancy in women". Our finding may point toward a better understanding of the social and cultural mechanisms of obesity in couples. Most couples seem to be spending more free time with their children. With increasing number of children, couples may have little time to spend on health behaviors. For example, they don’t go to the gyms and exercise centers.

3. It is unclear what is meant by â##â#selecting the cluster was made systematicallyâ## (top of p5). The correlation between couples is correctly accounted for by the GEE model, but consideration of whether the cluster sampling should also be accounted for is required.

* The explanation for the reviewer is as bellow:

SAMPLING
This article is based on the results of the Second Health Survey in Iran (2nd NHS). All citizens living in Iran were considered as survey population. A household was defined as those living in the same residence, participating in the household’s expenses and usually eat together. Any individual living single was also considered a household. The statistical framework used, was based on the households’ list available with every Health Department in the provinces, usually updated annually. Sampling was conducted on the basis of the cluster method, each cluster comprising of 8 households. The process was as follows: first of all, based on population size and one-day performance capacity of the data collection group, number of clusters was identified; second, the population size was divided to number of clusters to find out the systematic cycle; third, by using simple random sampling, the first cluster was chosen from the first cycle; forth, rest of clusters were systematically selected from the rest of households’ list.

CORRELATION
a) We focused our study on the respondents who lived in Tehran Province only.
b) Tehran is a capital city and people who lived in Tehran are relatively more homogeneous than those living in whole Iran.
c) We excluded: 1) the households with a pregnant woman inside; 2) those households that do not include a father or a mother; 3) those households with a single person inside, from the analyses. Hence, the cluster size is reduced.
d) It was found that the correlation within households is much higher than that of between households.

As the result, we believe the right choice is to apply GEE model and consider the correlation between couples, rather than ignore it and instead, consider between households correlations. We checked this idea using SAS PROC GENMOD and considered couples, nested in households, as correlated data and realized that when couples are considered as clusters, it is not needed to consider households’ clusters.
is the focus of the paper. Delete the first 2 sentences of the conclusions as the conclusions should relate to the findings from this specific study.

* The first line of the results of the abstract has been changed as the reviewer indicates.


* We have now included two papers (N0 35,36,41) in the References section as the reviewer indicates.

3. I think the sentence ending â##â#in recentâ## (without a full stop) (p3, line 7) should read â##recent years.â##

* Change made as indicated by the reviewer.

4. The sentence beginning â##Smoking cessation,â## (p3, start of paragraph 4) should be corrected to â##which fortunately often occurs once women learn of their pregnancyâ## rather than â##which fortunately occurs when their learn of pregnancyâ##.

* Change made as indicated by the reviewer.

5. It is stated (p5) that BMI was examined using the four WHO categories, but analyses only appear to be based on a binary outcome (obese versus not obese).

* The following statements now appear in the first paragraph of the measurements subsection of the Methods section:

BMI (Body Mass Index), our dependent variable, was calculated as weight in kilograms divided by the square of the height in meters (kg/m²), and subjects were classified into obese (BMI ≥30 kg/m²) and nonobese (BMI <30 kg/m²).

6. Delete â##physical activityâ## from the first sentence of the final paragraph on page 5 as it does not relate to the economic factors being discussed in the rest of the sentence. Physical activity should be dealt with in a separate sentence, probably in the paragraph relating to alcohol consumption.

* The following statement now appears in the fourth paragraph of the Measurements subsection of the Methods section:
We had no information on physical activity and alcohol consumption (the consumption of alcohol is prohibited in Iran).

7. The definition of active workforce should be edited for clarification purposes.

* The following statements now appear in the sixth paragraph of the Measurements subsection of the Methods section:

Workforce status: The original questionnaire item included twelve response categories. For this analysis, housewife, student, retired and unemployed were grouped into one category named "nonactive workforce" vs. "active workforce" (others).

8. It is stated that the strength of the association between number of children and obesity increased. Should this read the odds of obesity increased with increasing number of children?

* Change made as indicated by the reviewer.

9. Reword or delete the sentence (p8) Patterns that could explain this association in men are not detected.

* We have deleted the sentence as the reviewer indicates.

10. Clarification of the meaning of the coefficients presented in Table 2 would aid understanding of the results. Better use of footnotes and a clearer description of the models fitted in statistical methods section and/or results are required. How the different coefficients for male and female were obtained (i.e. presumably by interaction terms) should be clarified. In particular male vs female: active workforce and male vs female: nonactive workforce are hard to interpret and the description in the footnote should be improved.

* The reviewer is correct and we have added statistical analyses subsection to the Methods. The following statements now appear in the first paragraph of the statistical analyses subsection of the Methods section:

   Economic index, the number of children and age were examined as continuous variables. The other five variables were (0,1) variables. We looked specifically for interactions between sex and all covariates (sex × education, sex × smoking and sex × place of residence were not significant and we excluded these nonsignificant terms). The interaction between sex and number of children was not significant but it is sensible to include a variable that is central to the purpose of the study and report its estimated effect even if it is not statistically significant. A model containing interaction terms implies that both the estimated odds ratios and their corresponding confidence intervals vary as the values of the effect modifiers vary. A recommended practice is to use “typical” or “representative” values of variables, such as their mean values in the data for each variable.

* We have added these sentences to the finest paragraph of the Results:

Among subjects with active workforce level, the odds of obesity for men was 1.21 times that for women. In contrast, among subjects with nonactive workforce level, the odds of obesity for women was 2.44 times that for men.
* The footnotes of Table 2 now appear as:

a Body mass index 30 (kg/m²) or greater
b The interaction terms were included as cross-products between sex and the predictors of interest (number of children, age, economic index and workforce level) therefore, different OR for male and female were obtained.

' Number of children, age and economic index were examined as continuous variables. Male vs. female OR evaluated at the overall mean number of children, age, and economic index.

d Among subjects with active level, the odds of obesity for men was 1.21 times that for women.

e Among subjects with nonactive level, the odds of obesity for men was 0.40 times that for women.
The manuscript is aimed to investigate an interesting association between the number of children and obesity in couples, but it has fundamental limitations in the analysis carried out that make the research unable to test this hypothesis. Fundamental limitations
1. Although the objective of the research seems to be the effect of number of children on obesity in couples, the analysis is not directed to couples, but to women and men separately. The effect of number of children on obesity in women has not interest, since there are many papers showing this very well known association. If the authors have information from couples (as it seems to be), I strongly suggest a new analysis defining obesity (dependent variable) in couples rather than in individuals; for example, at least one member of the couple as obese, one member obese and the other at least overweight, etc, and with this â##operative individualsâ##, investigate the effect of number of children on obesity.
At least, authors have to consider this other way to reanalyzed the data: investigating the effect of number of children in men accounting for personal characteristics (not only socio-demographic, and life style factors but also obesity) also for the women of the couple. This kind of models would include some new variables assigned to the individuals analyzed (men) but based on information from the women in their couples. This may have interest, since the association in men is less known than in women and this approach would be able (in same way) to account for the effect of couple.

*Unfortunately, we had expressed the aim of our paper wrongly. But, as it is mentioned in the title and also in the background of abstract, we are interested to investigate the association between the number of children and obesity in men and women who are couples and also interested to compare these associations. We believe the reviewer is correct if we were interested to find out the association between number of children and couples' obesity. We also believe this mistake was due to our poor explanation about the aim of our manuscript. Therefore it is not needed to use the suggested statistical models.

* These sentences now appear in the background of abstract:
To date, few studies have assessed the association between the number of children and obesity in men and women who are couples. We aimed to investigate and compare this association in Iranian men and women aged 20-75 years.
We aimed to investigate the association between the number of children and obesity in men and women who are couples by using cross-sectional data from the National Health Survey in Iran.

2. Information on diet is not available for the analysis. The authors should discuss in detail the effect of that on the estimations.

* The reviewer is correct. There is not any information on diet for the analysis. But we believe since we have chosen the men and women that are couples and, as the result, are similar in many behaviors, diet has same effect on association between number of children and obesity in men and women of our sample. In such a situation we are able to compare these associations. In addition, dieting is often considered the best way to lose weight, but even with encouraging short-term results [1] most dieters do not maintain their new weight for long [2]. From 50% to 80% of dieters experience a frustrating “rebound” after weight loss [3] and [4]. The long-term effectiveness of dieting in weight loss is doubtful.


* This sentence now appears at the end of sixth paragraph of discussion section: We also had no information on diet.

Minor points
1. Methods section should explain in more detail the variables, their categories and the way in which they are included in the models (operative variables).

   * The reviewer is correct and we have added these sentences to the final paragraph (page 5) and first paragraph (page6):

   There wasn’t any information on household income, but economic index is a surrogate for household income. Some studies use self-reported income data, and education or occupation have been used as controlled background variables only. Self-reported income is subject to nonresponse and reporting bias. Economic index was defined as the area of living place (in square meter) divided by number of people in household.

   Workforce status: The original questionnaire item included twelve response categories. For this analysis, housewife, student, retired and unemployed were grouped into one category named "nonactive workforce" vs. "active workforce" (others).

   * The reviewer is correct and we have added "Statistical Analyses" subsection to the Methods section. The following statements now appear in the first paragraph of the Statistical Analyses subsection of the Methods section:

   Economic index, the number of children and age were examined as continuous variables. The other five variables were (0, 1) variables. We looked specifically for interactions between sex and all covariates (sex × education, sex × smoking and sex × place of residence were not significant and we excluded these nonsignificant terms). The interaction between sex and number of children was not significant but it is sensible to include a variable that is central to the purpose of the study and report its estimated effect even if it is not statistically significant. A model containing interaction terms implies that both the estimated odds ratios and their corresponding confidence intervals vary as the values of the effect modifiers vary. A recommended practice is to use “typical” or “representative” values of variables, such as their mean values in the data for each variable.

2. Numeric variables, such as age and especially number of children should be included in the models as categorical. The models run much better. The inclusion of quantitative variable is complementary and indicated trends.

   * Categorizing a continuous variable is known to result in the loss of information, lower statistical power and lower reliability. (see: Henian Chen, Patricia Cohen, Sophie Chen.. Biased odds ratios from dichotomizing of age. Statistics in Medicine 2007; 26:3487-97.)

3. Results presented (and discussion on its) have to focus on the association of interest. Most of the results presented (and the discussion) are concentrated on variables of control (education, smoking, etc.) and its relation to obesity, which is not the objective of the manuscript.
The reviewer is correct and we have added these sentences (second, third and fourth paragraphs) to the Results:

We started by fitting a preliminary GEE model including only sex and number of children to observe the influence of the potential confounders on obesity. This model shows that the odds of obesity increased with increasing number of children: unadjusted obesity odds ratios were 1.29 (95% CI: 1.21-1.39) and 1.10 (95% CI: 1.001-1.20) for women and men, respectively. The unadjusted OR of 1.29 for women was significantly different from the unadjusted OR of 1.10 for men because the interaction between the number of children and sex was statistically significant (p=0.006). In GEE model controlling for age, economic index, workforce status, education level, place of residence and smoking status, the odds of obesity increased with increasing number of children: obesity odds ratios were 1.18 (95% CI: 1.05-1.34) and 1.16 (95% CI: 1.04-1.29) for men and women, respectively. For each additional child, the odds of obesity increased by 18% among men and 16% among women. Our test of interaction by sex showed that the association between the number of children and obesity was not different among men and women (P=0.79).

* please see: response to 5th comment

4. The authors discuss the effect of fast-food on the association of interest, but they have no information on that.

* The reviewer is correct and we have added some sentences to discussion section. Please see response to 5th comment.

5. The discussion should go deeply on the matter of the research.

* The reviewer is correct and we have added these sentences (second, third and fourth paragraphs) to the Discussion:

Comparing two models the influence of the potential confounders included in analysis can be observed. Unadjusted ORs of 1.29 and 1.10 are changed to ORs of 1.16 and 1.18 for women and men, respectively. After controlling for potential confounders, the similar associations were seen in both sexes suggest a lifestyle, rather than a biologic, impact of having children in women. Our findings were consistent with the conclusions of Lawlor et al. [35] and Hardy et al. [36]. Lawlor et. al [35] concluded that "Lifestyle risk factors associated with child-rearing lead to obesity and result in increased CHD in both sexes; biological responses of pregnancy may have additional adverse effects in women". Hardy et al. [36] showed that "Any association between number of children and CHD risk factors is a result of lifestyle and behaviors associated with family life rather than being as result of the biological impact of pregnancy in women". Our finding may point toward a better understanding of the social and cultural mechanisms of obesity in couples. Most couples seem to be spending more free time with their children. With increasing number of children, couples may have little time to spend on health behaviors. For example, they don’t go to the gyms and exercise centers. It is
possible that the mechanisms are related to sedentary lifestyle and food habits. It is also possible, weight gain prevention programs in couples with increasing number of children are not successful even the format of programs may influence the willingness of subjects to participate [41].

6. Is information on age of children available? This may have strong influence on obesity of the couples and should be accounted for in the models.

   * No. The following paragraph has already appeared in Discussion section:
     There are some limitations of our study. This study is a cross-sectional study, which means that we cannot draw definitive conclusions concerning the direction of causality. However, this should be confirmed by further longitudinal studies. Measures of “the age of children” and “the length of time the children lived in the household” are useful in this regard but they were not available for study members.

7. Tables should show in more details the distribution of the main variables (dependent, the main independent and the control variables) in couples.

   * Since the model that has been offered by the reviewer is not used, we believe no change in tables is needed.