Author's response to reviews

Title: Prevalence of cardiovascular disease risk factors among schoolchildren in large urban areas of Turkey: directions for public health policy

Authors:

Bike Kocaoglu PhD (kocaoglb@boun.edu.tr)
Maria Dimitriou (ds20009@hua.gr)
Maria Kolotourou (ds20019@hua.gr)
Yasar Keskin (keskinyasar@yahoo.com)
Haydar Sur (haydars@yahoo.com)
Osman Hayran PhD (hayran@yahoo.com)
Yannis Manios (manios@hua.gr)

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Author's response to reviews: see over
Re: Corrections on Manuscript ID: 1702607176396086:
"Prevalence of cardiovascular disease risk factors among children of different socioeconomic status in Turkey. Directions for public health and nutrition policy"

The title has been revised to:
“Prevalence of cardiovascular disease risk factors among schoolchildren in large urban areas of Turkey: directions for public health policy”

Athens 25-8-2004

Dear BioMed Central Editorial Team,

Please find attached our revised manuscript and a letter indicating the changes made in the manuscript according to the points raised by the reviewers.

We would like to thank the reviewers for their useful and helpful comments on our manuscript. We hope that all the suggestions and corrections have been incorporated appropriately.

Sincerely yours,

Yannis Manios
1. Abstract: The first set of results presented is of lower total and LDL c among those form low SES compared to those form higher SES. Further, girls from higher SES have higher fat intake compared to those from lower SES. Despite these results, which suggest that cardiovascular disease risk profiles are actually worse among those children form higher SES compared to those from lower SES backgrounds, the authors conclude that their results show an association between adverse lipid profiles and physical inactivity and low SES. This does not make any sense. I think that part of the problem is the inappropriate switching from describing the results in terms of difference between low and medium and high SES groups and then between high and medium or low SES groups. A standard approach should be used to present the results and the authors need to be clear that their conclusions follow on from their results.

The suggested changes have been incorporated as requested (page 2), taking into consideration the results from the univariate analysis of variance that you have requested in your comment No 5.

2. Page 4 second paragraph second sentence: ‘Many recent reports have identified socioeconomic status (SES) differences, as another important parameter that appears to influence the accelerating prevalence of CVD risk factors in both developed and developing world [10-12]. It isn’t correct to say that SES differences are a cause of increasing CVD risk factor prevalence. At an individual level studies in contemporary developed countries have found CVD risk profiles and risk of CVD to be greater among those form lower SES background compared to higher SES backgrounds. SES-health outcomes associations are dynamic over time and between places and in contemporary developing countries and in earlier time periods for developed countries some of these associations are actually in the opposite direction. The authors should present a much more comprehensive summary of this literature in their introduction than they have and bring into the introduction (with appropriately citations) examples of the dynamic nature of SES-outcome associations. This would give a valuable context for why it is of interest (to an international audience) to have a publication of cross-sectional SES-CVD risk factor associations from Turkey.

The suggested changes have been incorporated as requested (page 3-4).

3. Method: SES assessment. The authors mention collecting data on a number of SES variables, namely family size, parental academic qualifications, occupation and financial holdings. They then define an SES variable that is based solely on dichotomised variables of educational attainment of the head of the household and car access. What is the justification for this? Why only these two SES characteristics? and Why combine them into one variable? This makes no sense to me at all. Different measures of SES have different meanings and can affect outcomes in different ways. Reporting and interpreting these differences can help in understanding the pathways between SES and health outcomes. Further, it is important for measures that are not binary to determine whether there are graded associations across the whole SES distribution. I think that the results should be presented for each measure of SES separately – that is family size, parental education, parental occupational social classification, finance and car ownership. For comparisons between different measures relative indices of inequality could be generated (see Mackenbach JP, Kunst AE). Measuring the magnitude of socioeconomic inequalities in health: an overview of available measures illustrated with two examples from Europe. Soc Sci Med 1997;44:757-71).
The authors used these two components of SES, because the inter-reliability of the other SES factors was very poor. In addition, there were a lot of missing values, regarding the other indices of SES including maternal educational level. Furthermore, judging by the experience of the authors living in Turkey, academic qualifications in Turkey are not always linked with certain occupational status. Consequently, a combination of educational status and financial index, such as car ownership were considered appropriate. A relevant comment has been incorporated as in SES assessment section (page 5-6).

4. There is too much emphasis throughout the results and conclusions on p-values rather than the magnitude of the effects, which is the important issue rather than the p-value. Also I don’t like the ANOVA approach and then different comparisons between the categories. In most other studies SES associations are graded across the distribution and therefore the appropriate analyses would be to define a consistent reference group (either the lowest or highest group) and then present mean differences (ratios of geometric means or proportionate changes for skewed variables) for the other categories and p-values for linear trend. The current results as well as being difficult to follow, emphasizing p-values rather than actual effect differences, feel like data dredging.

The suggested changes have been incorporated as requested (pages 9-11). Moreover, the further analysis of the data and the interpretation of the outcomes have been added to the Results section, as suggested in your comment No 5 (pages 9-11).

5. It would be valuable to conduct multivariable analyses to see how much of the variation in lipid profiles is related to SES differentials in dietary and exercise patterns. This would be the appropriate way to see if these variables do indeed explain differences in the lipid profiles.

We thank the reviewer for this useful comment. Relative analysis has been incorporated in the Results and Tables section as requested (pages 9-11 and Table 2).

6. Page 12 and 13 the authors conclude that the ‘more favorable HDL-C values observed for the high SES children compared to their medium and low SES counterparts and the opposite finding observed for the TC and LDL-C… could be attributed to the different levels of physical activity and to some extent to possible differences on dietary patterns.’ This conclusion does not follow on from the results presented. Firstly, I cannot see how exercise and dietary patterns could explain both good and bad lipid profiles at the same time. That makes no sense. Secondly the authors have not undertaken the appropriate multivariable analyses to determine whether physical activity and dietary factors do explain the SES lipid profile associations.

Relative comment has been incorporated in the Discussion section as requested, taking into consideration the results from the analysis requested in your comment No 5 (pages 11-12).

7. The discussion needs to consider the meaning of the results in more detail i.e. why do the authors think that those from lower SES have advantageous lipid profiles with respect to LDL-C and TC but disadvantageous lipid profiles with respect to HDL-C. How could these opposing results be used to develop sensible policy regarding health
inequalities? Further, they should give a fuller discussion of other studies from a variety of countries and times and put these results into the context of the changing nature of SES-outcome associations across time and between places. Relative comment has been incorporated in the Discussion section as requested, taking into consideration the results from the analysis requested in your comment No 5, which have revealed no opposing results regarding biochemical indices.

8. There is no discussion of the study limitations, including: (i) its cross sectional nature and therefore the possibility of reverse causation, regression dilution bias and survivor bias; (ii) the lack of data on other important CVD risk factors notably measures of adiposity, fasting glucose and insulin (which might help to explain the odd results with respect to lipids) and smoking; (iii) although the response proportion at 79% is good it is possible that this varied by SES and the authors should discuss the possible implications of this. Relative comment has been incorporated in the Discussion section (page 12), as requested.

9. There are spelling and grammatical errors throughout the manuscript that should be corrected. The manuscript was revised for grammatical errors and corrected. However, if there are any additional or specific observations, please let us know.