Author's response to reviews

Title: Risk behaviors and sports facilities do not explain socioeconomic differences in childhood obesity: a cross-sectional study

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Version: 2 Date: 29 September 2014

Author's response to reviews: see over
Author's covering letter for initial submission

Title: Risk behaviors and sports facilities do not explain socioeconomic differences in childhood obesity: a cross-sectional study

Authors:

Version: 1 Date: 25 September 2014

Comments: see over
Dear Editor:

I have the pleasure of enclosing a paper entitled, RISK BEHAVIORS, SPORTS FACILITIES AND SOCIOECONOMIC DIFFERENCES IN CHILDHOOD OBESITY: A CROSS-SECTIONAL STUDY, which we believe may be of interest to your journal's readership.

We sent this article and it was returned with reviewers' comments (MS:6242152713389728). Furthermore we were advised that if he suggested changes were made, it could be sent back again. This is the option we have chosen, therefore we resend the article to your journal describing the amendments made.

There is an increasing amount of literature on the influence of area-based socioeconomic environment on childhood obesity, with the majority of studies supporting the existence of such a relationship. The authors of these studies attribute this finding to the fact that children and adolescents living in areas with more adverse socioeconomic environments are more likely to have obesity risk behaviors and less access to infrastructures that promote healthy behaviors.

Most of these studies have been undertaken in the USA and UK but, until now, no research has addressed this topic in a Southern European city. This study, conducted in the city of Madrid, shows that, as has been observed elsewhere, there is an important relationship between the socioeconomic context of neighborhood of residence and childhood obesity. This relationship is not, however, accounted for by family socioeconomic position, risk behaviors or the availability of sport facilities.

We hereby confirm that this material has not been published in or submitted to another journal. We also confirm that all of the authors have made:

- Substantial contributions to the conception and design of the study and analysis and interpretation of the data
- Assisted in drafting the article and revising it critically for important intellectual content
- Given approval for the final version

Conflict of interest: None

I look forward to hearing from you.

Description of the changes made:

Replies to Juan Luis Gutierrez-Fisac

The research deals with a relevant public health topic and tested a convenient hypothesis. Authors analyze a representative sample of young people living in Madrid and they used advance statistical methods to test the hypothesis. The results are clear and make a contribution to the field. I believe that, this research deserve publication after discretionary revision:
Results
Comments on table 2 (page 8, lines 4 and 5) should mention that the significant relationship of obesity with fruit and/or veg consumption was inverse. Comment on page 8, line 24 is incorrect: "With regard to obesity, moreover, that magnitude of the odds ratio of the fourth quartile showed no significant difference with respect of the first quartile, which was used as reference."

AUTHORS
We agree with the reviewer. In the new version, the section has been re-written. Specifically, in the next way:

“Table 2 shows the prevalence of overweight and obesity according personal characteristics and risk behaviors. For prevalence of obesity significant differences were found according age and intake of fruit and vegetables, and for prevalence of overweight significant differences were found according age and sex. In the case of prevalence of obesity according intake of fruit and vegetables the magnitude was higher in those showing higher intake.”

In the same way we have modified the incorrect comment. In the new version the comment is the next:

“...Moreover, the magnitude of the odds ratio of the fourth quartile of per capita income showed significant differences with respect to that of the first quartile, which was used as reference.”

On results of table 4, it should be mention that, although there was no significant association, the magnitude of the OR was, similarly to the results in table 3, greater in the third quartile.

AUTHORS: In the new version we have added the next:

“The odds ratio of greater magnitude was observed in the third quartile of sports facilities, although none of the odds ratio was significant. The age and sex adjusted odds ratio for the areas with the lowest number of sports facilities with respect to the areas with the highest number of sports facilities was 1.25 (0.62 to 2.79). The adjustment for other variables decreased the magnitude of the odds ratio.”

Discussion
The measurement of diet used in the research, exclusively based on a two categories of frequency of consumption of fruit and vegetables, should not have permitted to conclude that "unhealthy diet" (page 12, line8) do not explain the association found. This absence of effect of diet on obesity-socioeconomic context association is also mentioned at the beginning of the discussion section (page 9, line 13-14). I suggest to the authors to moderate these comments and include a simply comment in the discussion (may be as limitations) on the raw measurement of diet and the possibility of confounding due to this important obesity related risk behavior.

AUTHORS
We agree with the reviewer. In the new version we have modified the comment at the at the beginning of the discussion section:

“...Overweight- and obesity-related risk behaviors measured in this study failed to explain....”
Also in the conclusion we have modified the conclusion:

“...This association is not accounted for by family socioeconomic position and measures used on unhealthy diet or physical inactivity..."

Finally, in limitations we have added the next:

“...In any case, the validity of instruments of measurement about diet and physical activity may not be appropriate. The measurement on diet is raw and in the measurement of physical activity has not been determined whether participants are meeting the recommendations of physical activity, because the intensity and duration of activity is unknown. This limitation may explain the absence of a gradient in these behaviors according socioeconomic context and therefore the null role in the association investigated.”

Tables
Problems with lineup in table 1

AUTHORS: Solved!

Replies to Katrina D DuBose
This study examined the relationship socioeconomic (SES) factors and physical activity has on the presence of childhood obesity in Madrid, Spain. Understanding the role SES factors and the built environment have in the development of obesity is necessary. This study is examining an area that is complex and needs further examination; however, there are some factors that need consideration before publication (See below).

Major Compulsory Revisions
Abstract
1.Page 2 lines 3-4, & 12: What is meant by household socioeconomic position? How is this different than socioeconomic context of the neighborhood? Is it really possible to have mixed SES housing – for instance, a low SES person living next door to a high SES person? Usually housing is in groups were there will be areas of low SES and areas of higher SES and the two will not be mixed. 3. Line 6: state what SES indictors are being used.

AUTHORS: We have made a mistake in using the term household socioeconomic position to refer to the socioeconomic position of the parents. As the reviewer says, it is rare a person of low socioeconomic position live next door a person of high socioeconomic position. In our paper we want to refer to socioeconomic position of parents, because in every neighborhood there are people of high and low socioeconomic position. We looked at the terms used in papers similar to ours and socioeconomic markers include one or more family (parent education, parent occupation, family income) indicators and area-level (postcode household, school or neighborhood socioeconomic index) indicators. So in the new version we have decided to use the term family socioeconomic position.

4. Line 10: define tertiary education for the reader.
5. Lines 11-15: When reporting odds ratios it is also important to report the 95% CI.

AUTHORS: In the new version we have included 95 CI of odds ratio.

6. The results of the abstract are a bit misleading. It appears that these odds ratios are significant, but when the tables are consulted the 95% CI include 1.0, so these are not statistically significant results. So the results and conclusions need to be re-written to match the data.

AUTHORS: Ok. In the new version both sections have been modified in the next way:

**Results:** Children and adolescents living in neighborhoods having lower per capita incomes or lower population percentages with university education had age- and sex-adjusted odds ratios (ORs) of overweight that were 1.84 (95%CI, 1.03-3.29) and 1.68 (0.95-2.94) times higher, respectively; after adjustment for household socioeconomic position, unhealthy diet and physical inactivity, these ORs fell to 1.80 (0.99-3.29) and 1.56 (0.87-2.79), respectively. In the case of obesity, the age- and sex-adjusted ORs in these quartiles of both indicators of socioeconomic context were 3.35 (1.06-10.60) and 3.29 (1.03-10.52), respectively, rising to 3.77 (1.12-12.70) and 3.42 (1.00-11.68) after adjustment for the remaining variables. The highest OR was observed in the third quartile, except in the case of the relationship between per capita income and obesity. No association was in evidence between the number of sports facilities per 1,000 population and physical inactivity.

**Conclusion:** The socioeconomic context is associated with obesity but not with overweight children in Madrid, although this relationship is not accounted for by household socioeconomic position, risk behaviors or availability of sports facilities.

**Background**

7. Page 3, line 12: Define what is household socioeconomic position.

AUTHORS: As we have commented previously, we have used the term family socioeconomic position.

**Methods**

8. Page 5, Lines 16-25: Please provide information on the reliability and validity of the physical activity measures used.

AUTHORS: Following the suggestion of reviewer, in the new version we have included the new comment:

“This measure of physical activity has previously been used and has shown a pattern by age, sex and socioeconomic status similar to that observed in studies of other countries in which they have used a different instrument of measurement.”
9. Page 6, line 14: What is tertiary education? On page 5 lines 10-14 education level is discussed, but education as only explained as having 2nd cycle secondary or higher OR less than 2nd cycle education. For readers who are not familiar with the Spanish education system of education the phrase tertiary education is meaningless. Plus why focus on tertiary education if on page 5 education is defined as 2nd cycle or lower?

AUTHORS
We agree with the reviewer. We have confused to reviewer because we have not adequately defined the indicators of family socioeconomic position based on education. In the new version we have noted that the indicator of family socioeconomic position has been divided into two categories: 2nd-cycle secondary and postsecondary and university education, and less than 2nd-cycle secondary education.

As one of the indicator of socioeconomic position of the neighborhood we have used the percentage of people with university education as an indicator reflecting investment in human capital. In the new version we have included the following comment: “per capita income as indicator of wealth; and the percentage of the population with university education as indicator of human capital”

10. Page 7: Is a statistical program like SUDAAN needed to account for the complex sample design of the study and also to produce results that are representative of the population in Madrid Spain?

AUTHORS:
In the present study a multilevel analysis was conducted that takes into account two levels (individuals and neighborhoods). However, the weight component in the sample design only affected individuals. Therefore, the SAS procedure used in this study allows to obtain representative results for the population of children and adolescents in Madrid.

Results
11. Page 8, Lines 3-5: The chi-squared analysis cannot determine if a relationship exists between variables. It is testing to see if there are differences between groups. The summary of results is not correct. This section needs to be re-written.

AUTHORS: We agree with the reviewer. In the new version, the section has been re-written. Specifically, in the next way:

“Table 2 shows the prevalence of overweight and obesity according personal characteristics and risk behaviors. For prevalence of obesity significant differences were found according age and intake of fruit and vegetables, and for prevalence of overweight significant differences were found according age and sex. In the case of prevalence of obesity according intake of fruit and vegetables the magnitude was higher in those showing higher intake.”

12. Lines 7-20: Report 95%CI when stating odds ratios. The results for the odds ratios are a misleading. They are written to appear that the results are significiation, which according to the 95% CI they are not. It is not until line 24 on page 8 and line 1 on page 9 does the reader find out that there is no difference between the highest and lowest quartile for obesity and overweight.
AUTHORS
We agree with the reviewer. In the new version, we have added that the significance disappeared in some cases when adjustment was made for other variables. Also, we have included confidence intervals at 95%

Discussion
13. Page 9, lines 9-10: This summary of the results is contrary to what the data show. It needs to be re-written to reflect the results. Only for per capital income and obesity is this statement correct.

AUTHORS
We agree with the reviewer. In the new version we have modified the lines. Now it can be read the next:
“Our findings show that children and adolescents residing in Madrid neighborhoods with worse socioeconomic indicators show higher prevalence of overweight and obesity that those residents in neighborhoods with better socioeconomic indicators, although only the difference in prevalence of obesity according per capita income was significant”

14. Page 10, line 2: A high prevalence of overweight and obesity was found in the intermediate-low quartile (quartile 2). The prevalence isn’t really different from quartile 1 for obesity (income: quartile 1 =8.2, quartile 2=8.3; education: quartile 1 =8.3, quartile 2=8.4), but is higher for overweight (income: quartile 1 =27.0, quartile 2=33.8; education: quartile 1 =29.4, quartile 2=34.8). Another study also found similar results. Please provide some rational as to why this level of income and/or education has a higher prevalence.

AUTHORS:
We believe that the only honest explanation we can give is this we have included in the new version:
“…Possibly, some obesogenic features of the neighborhood of residence, not measured in our study, are more frequent in the intermediate-low than in the low quartile…”

15. Page 10, Line 6: the relationship between what two factors are being discusses here? I’m not sure what “context” is referring too.

AUTHORS: We agree with the reviewer. In the new version we have modified the explanation:
“In addition to adjusting for indicators of family socioeconomic position, our analyses were also adjusted for diet and physical activity because these are known to be possible confounding factors in the relationship between socioeconomic context of the neighborhood and BMI”

16. Lines 7-10: It is also possible that physical activity was poorly measured. On page 5 for the person’s activity level the highest option is doing physical activity or sport for one or more times per week. Currently recommendations for physical activity are 60 minutes daily for children and adolescents. According to the survey used, there is no way to determine if the participants are meeting physical activity recommendations, because no measure of duration is quantified and being active for 1 day is very different than for 7 days. Further, it is possible
that there is a gradient with physical activity and some activity is better than none, but meeting physical activity recommendations is better than some activity. Given how physical activity was measured it is impossible to determine if this gradient exists.

AUTHORS: We agree with the reviewer. In the new version we have included in the limitation section of discussion the next comment:

“...In any case, the validity of instruments of measurement about diet and physical activity may not be appropriate. The measurement on diet is raw and in the measurement of physical activity has not been determined whether participants are meeting the recommendations of physical activity, because the intensity and duration of activity is unknown. This limitation may explain the absence of a gradient in these behaviors according socioeconomic context and therefore the null role in the association investigated.”

16. Page 11, lines 14-16: Please explain why greater heterogeneity of facilities and SES profile would be a bad thing? If the sample is homogeneous then it would be very difficult to detect a relationship between two variables – if a relationship did exist.

AUTHORS: Here we have not explained well the idea. We believe that the analysis based on neighborhoods is more appropriate to detect an association that the analysis based on larger areas. When larger geographic aggregations are studied (eg states in USA or regions in Spain), there is more homogeneity between some areas and others in the availability of facilities because the estimates represents an average of the heterogeneity within the state or regions. Or the same can be said in relation to the socio-economic profile of the residents. In the new version we have changed the wording of this idea as follows:

“In addition, a neighborhood-based study avoids limitations posed by the larger geographic units. For example, when states, regions or provinces are studied, the units of analysis are more homogeneous in terms of availability of facilities and residents’ socioeconomic profile, and it is more difficult to detect a relationship between two variables if a relationship did exist.”

Minor Essential Revisions

Results
18. Page 7, lines 21-24: This sentence is hard for the reader to understand and follow. Please re-write and consider making it two sentences instead of trying to put so much into one sentence.

AUTHORS: We agree with the reviewer. In the new version elaborate two sentences.

“Table 1 shows the relationship between the characteristics of study subjects and area of residence on the one hand, and the two indicators of socioeconomic context on the other. Significant relationship was observed for overweight, obesity, primary family earner’s educational level and occupation, and number of sports facilities per 1000 population.”

19. Page 9, lines 4-5: provide a summary of the results for table 4.

AUTHORS: In the new version we have added the next:
“The odds ratio of greater magnitude was observed in the third quartile of sports facilities, although none of the odds ratio was significant. The age and sex adjusted odds ratio for the areas with the lowest number of sports facilities with respect to the areas with the highest number of sports facilities was 1.25 (0.62 to 2.79). The adjustment for other variables decreased the magnitude of the odds ratio.”

Discussion
20. Page 10, Line 19-20: What is meant by “the social context may be more destructured...”?

AUTHORS:
In the previous version we wanted to summarize briefly some aspects that presumably occur in neighborhoods with the lowest socioeconomic indicators. In the new version we have extended this idea as follows:

“....the social context is more precarious and institutional support system is not sufficiently structured...”

Page 11, line 4: Change “cannot” to “did not”

AUTHORS: Done!

Tables
21. Table 1: The title is not clear. For example what is “area” referring to? Also, effect sizes were not calculated, so why is this included in the title. The spacing for quartile 4 in Per capita income and % of population with tertiary studies is off. This makes reading the table difficult as the numbers do not line up with the rows. Finally, in the footnotes, parentheses ( ) are needed around the category listings for per capita income. Example: quartile 1 (<9,724.29).

AUTHORS: In the new version the errors have been corrected.

22. Table 2: The p-value for Obesity under educational level should be 0.062 not 0,062

AUTHORS: Done!

23. Tables 3 & 4: for all ORs and 95% CI commas are used instead of a decimal in the numbers.

AUTHORS: In the new version the errors have been corrected.

24. This is not correct. Instead of 1,00 it should be 1.00. Please fix all the values in these two tables.

AUTHORS: In the new version the errors have been corrected.

25. Page 24: This is an extra page.
AUTHORS: The extra page has been eliminated.

Discretionary Revisions

Background
26. Page 3, Lines 4-6: Suggest re-writing to say “…increases their risk of obesity in adulthood (2) and development of chronic diseases (3,4).

AUTHORS: In the new version we have rewritten as suggested by the reviewer

27. Line 10: suggest removing: “greater material deprivation”

AUTHORS: We agree with the reviewer. In the new version we have eliminated “greater material deprivation”

28. Line 20: suggest removing “greater material deprivation” and saying “lower income” instead

AUTHORS: We agree with the reviewer. We have replaced “greater material deprivation” by and “lower income”.

29. Page 7, Lines 14-15: suggest re-writing to: “finally, because physical inactivityis a risk factor….” There is lots of research to show that being inactive is a risk factor for obesity.

AUTHORS: Done !

Discussion
30. Page 11, line 20: Suggest changing “insofar as possible limitations…” to “As far as limitations….”

AUTHORS: Done !

Yours faithfully,

R. Albaladejo

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