**PEER REVIEW HISTORY**

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**ARTICLE DETAILS**

<table>
<thead>
<tr>
<th>TITLE (PROVISIONAL)</th>
<th>Impact of the economic crisis on children's health in Catalonia: a before-after approach</th>
</tr>
</thead>
<tbody>
<tr>
<td>AUTHORS</td>
<td>Rajmil, Luis; Medina-Bustos, Antonia; Fernández de Sanmamed, María-José; Mompart-Penina, Anna</td>
</tr>
</tbody>
</table>

**VERSION 1 - REVIEW**

| REVIEWER                    | Christopher B. McClure, MPH, PhDc  
|                            | Director of Research  
|                            | The Health Management Academy  
|                            | USA  
|                            | Doctoral Research  
|                            | University of Iceland  
|                            | Iceland |

| REVIEW RETURNED | 30-May-2013 |

**THE STUDY**

Tables 1-3 are based on improper analysis. There are no tests of significance between either group (i.e. 2006 vs. 2010-12). There are differences in means and proportions, but we have no way of knowing if they are statistically significant means, proportions. The focus of the Results and, therefore, the conclusions, should not be on Tables 1-3, but the regression Tables. Tables 1-3 should only serve as a descriptive point.

A rather important point was not implicit. Is this a cohort or are these separate groups? This should be explicit in the Methods.

The authors need to test the differences between the groups in Tables 1-3. It is clear that the groups are different, but the solution to this difference is not to weight them in the regression models. Instead, the authors need to adjust for demographics. As the groups are highly different - for instance, the 2010-12 is much more education (unweighted) - the regression models need to be adjusted for the typical demographic confounders.

Going further, the regression tables (4-5) are difficult to read. The reference groups should be indicated in the Tables and all models need to be adjusted for. All the models are unadjusted. The difference in outcomes could be the direct result of (a) secular trends and (b) the inherent demographic differences between the groups.

**REPORTING & ETHICS**

I don't seem to be seeing any information on ethical approval.

**REVIEWER**

| REVIEWER                   | Prof. Beatriz G Lopez-Valcarcel, Ph.D.  
|                           | University of Las Palmas de GC, Spain |

|                           | No conflicts of interest |
### THE STUDY

**GENERAL COMMENTS**

The conclusion that "Inequalities in childhood obesity and quality of life have increased with the economic crisis" does not derive from the empirical results. Please rewrite

In text you write: "The objectives of this study were to analyze changes in the family life conditions and socioeconomic status of children" but in the summary you refer to quality of life. This concept is vague in this context.

Page 6 lines 40-46. The first substitute is to replace the selected individual in the family, therefore the percent of first substitutes in the survey of 2011 is not a consequence of the method but a practical result derived from it. It that is the case, please rewrite this sentence: "ESCA 2010-12 was conducted following the same method of administration as the survey in 2006: 68% of proxy respondent interviews for children ≤14 years were carried out in the selected individuals, and 17% in the first substitute".

In page 7 you standardize the scales to that “Mean scores on these scales were standardized to a mean of 50 and 1 standard deviation (SD) = 10" Could you please justify that standardization method? Why not the standard one (Mean=0, SD=1)?

Page 8 line 3. Please, define how do you measure SES

Page 8 line 14. As your binary variable for employment status is defined as =1 if at least one of the parents in unemployed, maybe it would be a good idea to change its name with unemployed

Page 8, lines 37-42. The text would be more clear if you change the sentence "Interaction terms between SES, health-related factors, and study period were also explored to analyze the influence of changes in the study period on the outcome measures" with "Interaction terms between SES, health-related factors, and study period were also explored to consider the possibility of changes over time in the effect of SES and of health related factors on the outcome measures".

Page 8 lines 44-46. The sampling weights are included only for estimating variances or also to estimate the model coefficients? Please explain and justify

Table 1. You report some missing values in the variables in ESCA 2006. Are not any missing values in the ESCA 2011-12?

Page 9 lines 54-. The prevalence of obesity you report has been calculated using the sampling weights? This is necessary in order to infer to the Catalonian population.

Page 10. The sentence “The KS-10 showed higher mean scores (better) in 2010-12 (85.4; 84.4-86.0) compared to 2006 (81.0; 80.7-81.7), but lower scores in children with a maternal primary education (82.4; 80.6-84.1) and unemployed families (83.34; 81.89-84.9)”. According to these figures, there does not seem to be any significant difference between the total group of children and those with
unemployed parents (84.4-84.9 is in both CI for 2011-12). Please explain

Page 13. According to the results:

“Junk food consumption improved in 2010-12 in families with a maternal primary education level (beta, [B]= 2.85; 0.83 to 4.88, for the interaction term of survey by primary education level ) and never having breakfast decreased in unemployed families in 2010-12 (odds ratio [OR]= 0.33; 0.13-0.80; for survey by employment status).”

It seems that during the economic crisis inequalities in health are being reducing. Children of low educated mothers and children of unemployed improve their nutrition habits. The text in abstract does not reflect these results.

Table 4 (title or headlines): please write a text referencing the type of models as in table 5

In the discussion section you write: “This study shows that there has been an increase in social inequalities with higher levels of unemployment in families with lower level of education in Catalonia”. That result is not in the results section though. Please delete that sentence

Page 19 lines 7-12. The sentence “Although certain behavior showed an improvement in disadvantaged families in 2010-12, like junk food consumption and having breakfast before leaving home, an alarming increase in overweight/obesity was found”. It is true that you find an increase in obesity for the overall population of children since 2006, but the increase is not worse among families with low SES. In fact, the interaction terms of mother’s education in table 5, eq of obesity, are negative (OR<1 non significant). Please discuss

In my opinion the use of health services (table 6) could be excluded from the paper without losing interest as is not related to health. Is it good or bad for health to visit an specialist apart from the pediatrician? Oral health could be proxied with the number of decayed-bad teeth, but going to the dentist does not imply to improve oral health. The results in table 6, then, do not have clear implications in terms of improvement or worsening of health outcomes or in terms of met needs.

Page 19 lines 27-29 I am not so sure as you are that “it is clear that children’s living conditions have worsened in this 6-year study period, and this change has had impact on their health” What do you mean here exactly for living conditions? Please, define

### VERSION 1 – AUTHOR RESPONSE

Reviewer: Christopher B. McClure, MPH, PhD

The authors would like to thank the reviewer for his helpful comments, which have helped to improve the quality of the manuscript.

Comment 1. Tables 1-3 are based on improper analysis. There are no tests of significance between either group (i.e. 2006 vs. 2010-12). There are differences in means and proportions, but we have no way of knowing if they are statistically significant means, proportions. The focus of the Results and, therefore, the conclusions, should not be on Tables 1-3, but the regression Tables. Tables 1-3 should only serve as a descriptive point.
Re: In the revised Table 1 we have included statistical comparisons of the sample characteristics between 2006 and 2010-12 using the chi-square test and t test. Regarding Tables 2 and 3, the purpose of the study was to establish comparisons for the total sample between the 2006 and 2010-12 surveys and to compare the distribution of categories within each year of assessment (ie, to assess the differences between categories of a given health behavior in 2010-12). Hence, we believe it is preferable to leave the 95% confidence interval of the percentages and mean scores in these tables to facilitate identification and interpretation of the statistically significant differences between surveys and within each variable between the analyzed categories. Please see revised Tables 1-3 in the new version of the manuscript and the text in the Results section, which includes the significance level of Table 1 (page 9 first paragraph):

"The characteristics of the samples in ESCA 2006 and 2010-12 are shown in Table 1. Mean age was 7.9y (standard deviation [SD] 0.08) in 2006 and 6.9y (0.08) in 2010-12 (p<0.01). ESCA 2006 showed a lower percentage of children from families with a maternal university degree (22.8% vs. 29.5%, p<0.01), families of immigrant origin (9.5% vs. 20.1%, p<0.01), and unemployed families (9.1% vs. 20.6%, p<0.01)."

Comment 2: A rather important point was not implicit. Is this a cohort or are these separate groups? This should be explicit in the Methods.

Re: We appreciate that this omission has been pointed out. This study analyzes two cross-sectional surveys carried out by the Health Department of the Autonomous Government of Catalonia in representative samples of the general population. We have tried to better explain the study design in the Method section (page 6 first paragraph):

... "The study design is a before-after analysis of data from two separate cross-sectional representative samples of children in the 2006 ESCA survey (baseline, before the crisis) and the first 4 waves of ESCA 2010-12 (after start of the crisis)."

Comment 3: The authors need to test the differences between the groups in Tables 1-3. It is clear that the groups are different, but the solution to this difference is not to weight them in the regression models. Instead, the authors need to adjust for demographics. As the groups are highly different - for instance, the 2010-12 is much more education (unweighted) - the regression models need to be adjusted for the typical demographic confounders.

Re: As the Reviewer pointed out, the ESCA 2006 and 2010-12 samples differ in their sociodemographic characteristics and the differences are due to a various reasons. For example, the 2010-12 sample has a higher maternal education level than the ESCA 2006 sample. This difference reflects a change in the characteristics of the Catalan population, as the education level of women increased from 2005 to 2010, according to the Census data. Furthermore, according to the same Census data, unemployment increased from 2006 to 2012 at a higher rate than what occurred in ESCA 2010-12. These differences, as the reviewer says, may have had an influence on the results. However, this influence could bias the results in the direction of the null hypothesis, namely that better maternal education is associated with better health habits, health-related quality of life, and mental health. On the other hand, this is reflected in part in the results for the whole sample. Similarly, as the unemployment rate appears underestimated by ESCA 2010-12, the results may underestimate the impact on inequalities in children's health. We have tried to better and clearly explain these aspects in the revised version of the manuscript (see below). In contrast, the variable origin differs due to the form of data collection, which in 2006 was not as complete as in 2010-12. For this reason, the results derived from this variable should be viewed with caution, as was mentioned in the limitations.
Despite these limitations, we believe that the lack of cohort studies specifically designed to analyze the impact of the crisis on children and the scarcity of studies showing health outcomes in this population justify the relevance of the present study.

Weights were not taken into account to resolve the issues mentioned above but they were taken into consideration to restore the probability of selection of each individual in the sample and to estimate variance. These analyses were carried out using Stata 10.0, and the complex sampling design was accounted for by applying specific weights for each survey in the estimation of variance, as was stated in the statistical analysis section of the original version. All the multivariate regression models included all the variables simultaneously. For example, the logistic regression equation of obesity simultaneously included age, sex, educational level, year of assessment, the type of family, origin, chronic conditions, restriction of activity, health-related behavior, and interaction terms between year of assessment and educational level, unemployment, and health behavior. Thus, the results on the independent effects of each variable and the interaction terms can be interpreted, taking into account the limitations commented above.

We have expanded the section on the limitations of the study related to the comparison between samples in an attempt to analyze each difference in greater depth, and have added a reference on the census data (discussion, page 19 last paragraph):

… “Some limitations of the study deserve comments. Differences in the sample characteristics of the two surveys may have influenced the results. The ESCA sample 2010-12 was slightly younger and mothers were more educated. The results on the level of education reflect true differences in the general population from Catalonia. According to the census data25, the percentage of women with university degrees has increased between 2005 and 2010. These changes could be reflected in the better HRQOL found in the whole 2010-12 sample of the present study, although the figures could also be associated with secular trends. The percentage of unemployed households seems to be underestimated in ESCA 2010-12: in 11% of households all members were unemployed in 2010 according a previously mentioned survey22 whereas in ESCA 2010-12, the figure was 5%. These results may underestimate the impact of crisis on inequalities in children's health. Differences in the percentage of children from immigrant families could be attributed to data collection in 2006, which was less exhaustive with respect to the variable parents’ place of birth. For this reason, the results related to this variable should be interpreted with caution…….”

Comment 4: Going further, the regression tables (4-5) are difficult to read. The reference groups should be indicated in the Tables and all models need to be adjusted for. All the models are unadjusted. The difference in outcomes could be the direct result of (a) secular trends and (b) the inherent demographic differences between the groups.

Re: We have tried to improve the presentation of the multivariate models in the revised version by adding the reference category for each variable in the new Tables 4 and 5. As was mentioned above, all the models are adjusted for the remaining variables in the equation. This fact has been added as footnotes. The potential influence of factors, such as the secular trends has been included in the limitations of the study (please see the answers to a previous comment and revised Tables 4 and 5).

Comment 5: I don't seem to be seeing any information on ethical approval.

Re: We thank the Reviewer for this comment. The study was based on two comparable cross-sectional surveys from the Catalan Health Interview Survey (ESCA). The ESCA Statistical Plan is part
of the Government of Catalonia and is regulated by Decree 467/2004, of December 28, by approving the annual performance statistics from the year 2005. It is an official statistic, so selected individuals are likely to participate in the survey, and it meets all the regulatory requirements, in particular with respect the confidentiality of the information. The ESCA is conducted by the Department of Health of the Autonomous Government of Catalonia. All analyses using ESCA data must be anonymized, so that no individual information is identifiable.

We have included a paragraph in the methods section on this issue (page 6 first paragraph):

“The ESCA Statistical Plan is a part of the Autonomous Government of Catalonia and is regulated by Decree 467/2004 of December 28, according to which the performance statistics have been approved yearly since 2005. It is an official statistic, so selected individuals are likely to participate in the survey, which meets all the regulatory requirements, in particular confidentiality of the data obtained. The ESCA is conducted by the Department of Health of the Government of Catalonia. All analyses using ESCA data must be anonymized, so that no individual information is identifiable.”

Reviewer: Prof. Beatriz G Lopez-Valcarcel, Ph.D.

The authors would like to thank the reviewer for her helpful comments, which have helped to improve the quality of the manuscript.

Comment 1: The conclusion that “Inequalities in childhood obesity and quality of life have increased with the economic crisis” does not derive from the empirical results. Please rewrite

Re: The conclusions of the study have been rewritten to summarize the empirical results of the study. The abstract has been modified as follows:

... “Conclusions:
Although some health-related behavior improved during the study period, childhood obesity increased and inequalities in health-related quality of life appeared...”.

Comment 2: In text you write: “The objectives of this study were to analyze changes in the family life conditions and socioeconomic status of children” but in the summary you refer to quality of life. This concept is vague in this context.

Re: We suppose that this comment refers to the sentence in the abstract mentioned above (now corrected) and “Key messages” provided, which is where we included the sentence regarding quality of life. We agree with the Reviewer that the term quality of life is broad and vague. We analyzed health-related quality of life, which is a more accurate and recognizable term. According to this and previous comments, the key messages have also been rewritten to gain in precision regarding the results of the manuscript and to include health-related quality of life as the key variable. Key messages have been rewritten as follows:

... “Key messages
• An increase in social inequalities has occurred in Catalonia, with higher levels of unemployment in families with lower levels of education.
• Although some health-related behavior improved in the overall population in the period studied, inequalities disparities in childhood obesity remained and inequalities in health-related quality of life increased appeared according to the level of education.”

Comment 3: Page 6 lines 40-46. The first substitute is to replace the selected individual in the family,
therefore the percent of first substitutes in the survey of 2011 is not a consequence of the method but a practical result derived from it. If that is the case, please rewrite this sentence: “ESCA 2010-12 was conducted following the same method of administration as the survey in 2006: 68% of proxy respondent interviews for children ≤14 years were carried out in the selected individuals, and 17% in the first substitute.”

Re: The sentence has been modified according to the suggestion, as follows (page 6 last paragraph and 7 first paragraph in the revised version):

“ESCA 2010-12 was conducted following the same method of administration as the survey in 2006. In this second survey, 68% of proxy respondent interviews for children ≤14 years were carried out in the selected individuals, and 17% in the first substitute.”

Comment 4: In page 7 you standardize the scales to that “Mean scores on these scales were standardized to a mean of 50 and 1 standard deviation (SD) = 10” Could you please justify that standardization method? Why not the standard one (Mean=0, SD=1)?

Re: Most psychometric instruments measuring health-related quality of life, created or revised in the last decade and recently updated (e.g., the SF-36 and SF-12, standardize mean scores to an arbitrary mean of 50 and 1 standard deviation =10). This mode of standardization is used for example, to avoid obtaining negative scores and to facilitate interpretation of the results and their graphical presentation. Please see the Manuals of such widely used instruments as the SF-12v2 or KIDSCREEN for more information.

Comment 5: Page 8 line 3. Please, define how do you measure SES

Re: We regret the confusion. The measure for SES included in the study was maternal level of education. The sentence has been modified as follows (please see page 8, 2nd paragraph):

“Sociodemographic variables included age, sex, and family maternal level of education as a measure of socioeconomic status (SES).”

Comment 6: Page 8 line 14. As your binary variable for employment status is defined as =1 if at least one of the parents in unemployed, maybe it would be a good idea to change its name with unemployed

Re: In keeping with this comment, the variable employment status has been changed to unemployed in the revised version.

Comment 7: Page 8, lines 37-42. The text would be more clear if you change the sentence “Interaction terms between SES, health-related factors, and study period were also explored to analyze the influence of changes in the study period on the outcome measures” with “Interaction terms between SES, health-related factors, and study period were also explored to consider the possibility of changes over time in the effect of SES and of health related factors on the outcome measures”

Re: The sentence proposed by the Reviewer is more precise than our original statement. The sentence has been modified accordingly page 8 last paragraph):
“Interaction terms between SES, health-related factors, and study period were also explored to consider the possibility analyze the influence of changes over time in the effect of SES and health-related factors study period on the outcome measures.”

Comment 8: Page 8 lines 44-46. The sampling weights are included only for estimating variances or also to estimate the model coefficients? Please explain and justify

Re: We have applied weights in the estimations of coefficients and variance by means of STATA 10.0, as is usually recommended when the sample design is different from a random sampling selection. The sentence has been modified as follows (page 8 last paragraph in the revised version):

…“ considering the complex sampling design by applying specific weights for each survey in the estimation of coefficients and variance.”

Comment 9: Table 1. You report some missing values in the variables in ESCA 2006. Are not any missing values in the ESCA 2011-12?

Re: Missing values for both surveys have been included (please see footnote of Table 1)

“Missing values 2006: level of education (7); unemployment (56); 2010-12: type of family (3); unemployment (10).”

Comment 10: Page 9 lines 54-. The prevalence of obesity you report has been calculated using the sampling weights? This is necessary in order to infer to the Catalanian population.

Re: As was stated above, all coefficients have been calculated taking into account sampling weights. Thus, it is possible to make inferences related to the general population.

Comment 11: Page 10. The sentence “The KS-10 showed higher mean scores (better) in 2010-12 (85.4; 84.4-86.0) compared to 2006 (81.0; 80.7-81.7), but lower scores in children with a maternal primary education (82.4; 80.6-84.1) and unemployed families (83.34; 81.89-84.9)”. According to these figures, there does not seem to be any significant difference between the total group of children and those with unemployed parents (84.4-84.9 is in both CI for 2011-12). Please explain

Re: It is true that in the bivariate analysis there were no differences between children from unemployed families and the overall population in ESCA 2010-12, but there was a statistically significant difference comparing unemployed and employed families in ESCA 2010-12 (83.34; 81.89-84.9 vs 85.89; 85.18-86.6). The differences between employed and unemployed were not found in the ESCA 2006 sample.

Comment 12: Page 13. According to the results: “Junk food consumption improved in 2010-12 in families with a maternal primary education level (beta, [B]= 2.85; 0.83 to 4.88, for the interaction term of survey by primary education level ) and never having breakfast decreased in unemployed families in 2010-12 (odds ratio [OR]= 0.33; 0.13-0.80; for survey by employment status).” It seems that during the economic crisis inequalities in health are being reducing. Children of low educated mothers and children of unemployed improve their nutrition habits. The text in abstract does not reflect these results.
Re: We agree with the Reviewer that during economic crisis some inequalities in specific health behaviors and health status measures have improved, while other outcome measures showed worse results. These diversities in the results likely depend on several factors, such as the previous situation, the severity and duration of the crisis and the measures that governments, the society, and individuals provide to minimize the negative effects of the crisis. In this sense, the results mentioned by the Reviewer could act as a buffer against worsening results in terms of inequities in obesity, which is one of the main public health problems in childhood in Spain. According to the Reviewer’s comment, we have added these results, as follows (please see the abstract in the revised version):

“…Eating habits have improved in 2010-12 in disadvantaged families (ie, junk food consumption improved in families with a maternal primary education level; beta [B]=2.85; 0.83 to 4.88, for the survey interaction by primary education level)…."

Comment 13: Table 4 (title or headlines): please write a text referencing the type of models as in table 5 In the discussion section you write: “This study shows that there has been an increase in social inequalities with higher levels of unemployment in families with lower level of education in Catalonia”. That result is not in the results section though. Please delete that sentence

Re: We thank the Reviewer for this specific comment. Although the objectives of the study were “to analyze changes in the family life conditions and socioeconomic status of children (0-14 y) in Catalonia between the 2006 and 2010-2012 period; to study the association of these changes with modifications in health-related behavior, physical and mental health, and health-related quality of life (HRQOL)” in the original version, we omitted the data on changes in the unemployment status according to the maternal level of education. To rectify this omission, we have included a new table (Table 2 in the revised version) presenting family employment data for ESCA 2006 and 2012 according to the sociodemographic data. Unemployed families were more common among families with a primary education than the remaining families (please see new Table 2). The following changes have been introduced in the revised version: In the results section (page 9 last paragraph):

“Table 2 shows changes in unemployment according to sociodemographic variables. The percentages of unemployed families increased in ESCA 2010-12 (9.7%; 8.2-11.2 in ESCA 2006 and 20.7%; 18.8-22.7 in ESCA 2010-12). This change was particularly important in families with a maternal primary education (12.7%; 9.0-16.4 in 2006 to 36.3%; 30.0-42.6)."

Comment 14: Page 19 lines 7-12. The sentence “Although certain behavior showed an improvement in disadvantaged families in 2010-12, like junk food consumption and having breakfast before leaving home, an alarming increase in overweight/obesity was found”. It is true that you find an increase in obesity for the overall population of children since 2006, but the increase is not worse among families with low SES. In fact, the interaction terms of mother’s education in table 5, eq of obesity, are negative (OR<1 non significant). Please discuss

Re: We agree that the increase in the prevalence of obesity was proportional to the level of education, maintaining the same level of inequalities. Nevertheless, we think that obesity is one of the major health problems in our population of children and that independently of the improvement of these health behavior patterns, socioeconomic inequalities in obesity persisted. Moreover, attributable risk of maternal education for obesity increased given the high prevalence in children from families with a maternal primary education. According to this comment, the sentence in the Discussion has been modified in an attempt to be more precise regarding our findings, as follows (page 18 first paragraph):

“Although Certain behavior patterns improved in disadvantaged families in 2010-12, such as junk food consumption and having breakfast before leaving home. Nonetheless, an alarming increase in
overweight/obesity in the total population was found during the study period.”

Comment 15: In my opinion the use of health services (table 6) could be excluded from the paper without losing interest as is not related to health. Is it good or bad for health to visit an specialist apart from the pediatrician? Oral health could be proxied with the number of decayed-bad teeth, but going to the dentist does not imply to improve oral health. The results in table 6, then, do not have clear implications in terms of improvement or worsening of health outcomes or in terms of met needs.

Re: We thank the Reviewer for this interesting and important comment. We tried to analyze the use of healthcare services taking into account health needs, following a traditional method to assess equity in the use of healthcare services. Nevertheless, we agree with the Reviewer that in this specific case these analyses did not substantially add to the health status of this population. Hence, in the revised version we have excluded the analysis of healthcare service use. Therefore, all mentions of healthcare service use have been deleted from the Introduction, the objectives of the study, the Methods, Tables 3 and 6 of the Results, and some paragraphs from the Discussion.

Comment 16: Page 19 lines 27-29 I am not so sure as you are that “it is clear that children’s living conditions have worsened in this 6-year study period, and this change has had impact on their health” What do you mean here exactly for living conditions? Please, define

Re: We thank the Reviewer for this comment. As was mentioned above in an answer to a similar comment, we have included a new Table (Table 2) in the revised version with results on living conditions (unemployed families) and a comment on these results. We think that the sentence is now justified in the light of the changes in family living conditions and the positive and negative influences on children’s health, so it has been left in the revised version of the manuscript.

VERSION 2 – REVIEW

<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Christopher McClure</th>
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<tbody>
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<td></td>
<td>University of Iceland</td>
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REVIEW RETURNED 12-Jul-2013

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