PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Relationships between Social Spending and Childhood Obesity in OECD Countries: An Ecological Study
AUTHORS	Miyawaki, Atsushi; Evans, Charlotte; Lucas, Patricia; Kobayashi, Yasuki

VERSION 1 – REVIEW

REVIEWER	Charlotte Wright University of Glasgow
REVIEW RETURNED	23-Nov-2020

GENERAL COMMENTS	I liked this analysis and paper a lot. It addresses an important issue in a novel way, which seems congruent with the forgoing literature, though this is not a literature I am familiar with. The paper is well written and clear. I have only a few comments: Methods The authors have used a sensible stringent definition of childhood obesity (BMI >2SD) and adjusted for key covariates. The authors should be more explicit in the analysis about what were their primary outcomes - the cross section and longitudinal partial correlations and what was secondary (the disaggregated partial correlations) I am not clear why the disaggregated analysis was only done in the longitudinal data? If possible they should show results for the cross-sectional analysis also. For this analysis surely multiple regression would be more appropriate instead of, or in addition to partial correlation, - or was this limited by the sample size? Results These results also surely need some measure of spread around the regression lines – I think they should probably show R2 also I am not clear why there is so much emphasis on the association with change when the cross sectional data are just as plausible? Indeed the fact that different countries feature at extremes in the different analyses surely adds force to their finding. In the secondary analysis the authors need to more explicitly state that education and ECEC were the only significant components in univariate analysis . In the results (p13) and again in the discussion (p17) the authors refer to the association with family allowance as 'weak' when the p value =0.34. I can see that this is the only other factor that is inversely related, but as this is miles away from statistical significance it probably shouldn't be referred to in the text at all. Discussion The authors acknowledge the intrinsic limitations of an ecological analysis, but maybe should also emphasise the limited power they have with only between 29-35 subjects (countries) which

	presumably limited the range of possible multivariate analyses . The US is a striking outlier in the cross sectional, but not the longitudinal analysis. I think this merits some discussion – why might this be? Small things Page 10 lines 3-5 Replace “information” with “variables” “Furthermore, we examined several country-level sociodemographic information, including the population of children” P16 Don’t refer to r=0.5 as ‘strong’ in discussion – it is only moderate “Social spending on ECEC was strongly associated with reduced obesity growth rates for both girls and boys, while school education was strongly associated with reduced growth of obesity among girls.”

REVIEWER	Pierre Levasseur INRAE France
REVIEW RETURNED	30-Nov-2020

GENERAL COMMENTS	Review due for BMJ OPEN: Relationships between Social Spending and Childhood Obesity in High-Income Countries: More Welfare, Less Obesity? This article analyzes a relevant and interesting research question. The authors provided a notable work in merging several sources of second hand data for OECD countries. Their results show that public investments in education and ECEC are negatively correlated to childhood obesity, cross-sectionally (when absolute values are considered) but also longitudinally (relying on variation rates across time). However, I think that the manuscript can be improved as well as the analytical approach. My comments are below. Major comments 1. The description of the statistical methodology is unclear and should be rewritten with more efforts for transparency. In line 28-38 page 11 for instance, the authors mention several social characteristics available for each country in the database. Why all these variables are not used as control factors in linear regressions? Indeed, the partial correlation model only controls for 3 variables: GDP per capita, unemployment rate, and poverty rate. Why? Please justify your choice. 2. The introduction of further available characteristics will reduce potential estimation biases due to omitted variables. Specifically, one control factor is of particular concern: baseline rate of childhood obesity for each country. Indeed, omitting this control factor in your estimates, the authors probably understate the protective impact of social spending against childhood weight gain. Indeed, one can assume that a country with a high baseline obesity rate will invest more in anti-obesity policy and social programs than a country where obesity is low and not a particular issue. Controlling for baseline obesity rates, the estimates might be less influenced by heterogeneous effects, as argued by the authors in line 5-18 page 19. 3. Line 33 page 19: The authors write that their “findings were based on OECD countries’ data and might not be generalizable to low- and middle-income countries”. However, in Table 1, several higher-middle income countries are listed as being into the database (e.g. Hungary, Latvia, Mexico, Poland, Slovakia, Slovenia and Turkey). Hence, to investigate potential heterogeneous effects according to the level of a country economic development, I recommend the authors to run additional estimates for specific subsamples (high

	income OECD countries versus higher-middle income OECD countries). The results might be different and could significantly enrich the existing literature in development studies. 4. Line 50 page 14: Please report the table results for the partial correlation model. 5. Line 26, page 18: The authors confess in limitations that omitted factors may bias their estimates (e.g. concomitant anti-obesity public policies) and overstate the negative link between social spending and childhood obesity. I suggest to also mention other omitted factors that could understate this negative link, such as baseline variations in childhood obesity rates. Indeed, countries with high obesity prevalence may decide to highly invest in social programs as a reaction and not a prevention. Note that the latter bias can be controlled for by the authors, by including baseline childhood obesity rates in the set of explanatory variables in linear multivariate models. 6. Did the authors consider to use multivariate linear regression models, such as a pooled ordinary least square estimator (when cross-section data are used) and time fixed-effect estimator (when longitudinal data are used)? Such estimators are very powerful to estimate relationships controlling for a large set of further characteristics. Minor comments Line 50-54 page 7: The authors claim that low attention has been paid to the roles of social protection policies in childhood obesity prevention. However, there is a growing literature in development and health economics that analyzes the impact of social protection program and education-based investments on adult obesity (see for instance Levasseur 2019 in World Development; Nagano et al. 2020 in World Development; Etilé 2014 in Economic & Human Biology). The authors could mention these works to complete the literature review. Line 38-43 page 9: The sentence is unclear. Please reformulate. Line 47-52 page 9: The sentence is unclear. Please reformulate. Line 36 page 16: The authors may also refer to the results of Anderson and Butcher 2006 (published in Journal of Human Resources), which found that economic pressures for schools are vector of childhood obesity (needing to sell junk food and/or drop gym class to avoid bankruptcy). Line 12 page 17: Regarding gender-based differences in gym class programs, the authors may read and cite Cawley Frisvold and Meyerhoefer 2013, published in Journal of Health Economics.

VERSION 1 – AUTHOR RESPONSE

Comments by Reviewer 1
I liked this analysis and paper a lot. It addresses an important issue in a novel way, which seems congruent with the forgoing literature, though this is not a literature I am familiar with. The paper is well written and clear.

Thank you for reviewing our manuscript and giving us helpful feedback.

I have only a few comments:
Methods
1. The authors have used a sensible stringent definition of childhood obesity (BMI >2SD) and adjusted for key covariates. The authors should be more explicit in the analysis about what were their primary outcomes - the cross section and longitudinal partial correlations and what was secondary (

the disaggregated partial correlations)

The primary exposure was total social spending on children, and the secondary exposures were five dimensions of (disaggregated) social spending on children. We also conducted two kinds of analyses: 1) cross-sectional analyses for descriptive purpose and 2) longitudinal analyses to effectively investigate the association between social spending on children and childhood obesity within the same country. We made this framework more explicit in the manuscript as follows:

(Page 9, paragraph 3)
"Our primary exposure variable was total social spending on children,…"
(Page 10, paragraph 2)
"Our secondary exposure variables were five dimensions of social spending on children (family allowance, maternal and parental leave, ECEC, school education, and other benefits)."
(Page 11, paragraph 3)
"Next, we cross-sectionally investigated the relationship between total social spending on children and childhood obesity across OECD countries, using 2015 data."
(Page 12, paragraph 1)
"Then, to effectively investigate the association between social spending on children and childhood obesity within the same country, we examined the longitudinal trends in total social spending on children and childhood obesity during the period 2000−2015."

2. I am not clear why the disaggregated analysis was only done in the
longitudinal data? If possible they should show results for the cross-sectional analysis also. For this analysis surely multiple regression would be more appropriate instead of, or in addition to partial correlation, - or was this limited by the sample size?

Thank you for your suggestion. We emphasise the longitudinal analysis because focusing on the changes over time makes it possible to effectively investigate the association between social spending on children and childhood obesity within the same country (by controlling for unmeasured country-specific factors). Nevertheless, we are happy to add the results for disaggregated analysis using cross-sectional data with a multivariable linear regression model (we substituted it for partial correlation analyses) for descriptive purpose. We did not find any statistically significant associations with childhood obesity across all the dimensions of social spending for both sexes. We showed the results in Table 2 and the manuscript as follows (Page 15, paragraph 2):

"When we focused on the specific dimensions of social spending in cross-sectional analyses (Table 2), we found no evidence that either dimension of social spending on children was associated with the prevalence of childhood obesity."

Results
3. These results also surely need some measure of spread around the regression lines – I think they should probably show R2 also

We showed the R-squared for regression analyses in Table 2 and Table 3.
Both reviewers proposed to use multivariable linear regression models instead of partial correlation analyses for this study. We agree with the reviewers, and the findings were qualitatively unaffected even after we applied regression analyses (please see the Point 1 for the other reviewer for more details).

4. I am not clear why there is so much emphasis on the association with change when the cross sectional data are just as plausible? Indeed the fact that different countries feature at extremes in the different analyses surely adds force to their finding.

As stated above (Point 2), We emphasise the longitudinal analysis to effectively investigate the association between social spending on children and childhood obesity within the same country (by controlling for unmeasured country-specific factors). We clarified this point in the Method section.

(Page 12, paragraph 1)
"Then, to effectively investigate the association between social spending on children and childhood obesity within the same country, we examined the longitudinal trends in total social spending on children and childhood obesity during the period 2000−2015."

5. In the secondary analysis the authors need to more explicitly state that education and ECEC were the only significant components in univariate analysis .

Thank you for your suggestion. We clarified it as follows (page 15, paragraph 2):

"However, when we focused on the changes over time (Table 3), we found an inverse association between the change in spending on ECEC and in the prevalence of childhood obesity for girls ($\beta = -1.2 \times 10^{-2}$; $p = 0.045$) and boys ($\beta = -2.1 \times 10^{-2}$; $p = 0.049$). We also found an inverse relationship between the change in spending on school education and the growth in childhood obesity for girls ($\beta = -1.1 \times 10^{-2}$; $p = 0.01$), but not for boys ($\beta = -0.5 \times 10^{-2}$; $p = 0.43$). The change in social spending on family allowance, maternal leave and other benefits were not associated with the growth in the prevalence of childhood obesity for either sex."

6. In the results (p13) and again in the discussion (p17) the authors refer to the association with family allowance as 'weak' when the p value =0.34. I can see that this is the only other factor that is inversely related, but as this is miles away from statistical significance it probably shouldn't be referred to in the text at all.

We agree with the reviewer. Family allowance was not associated with statistical significance. Please see the answer to the Point 5. Also, we omitted the Discussion subsection where family allowance had been discussed.

Discussion
7. The authors acknowledge the intrinsic limitations of an ecological analysis, but maybe should also emphasise the limited power they have with only between 29-35
subjects (countries) which presumably limited the range of possible multivariate analyses .

The limited number of samples is also limitation of this study (the robust information on non-OECD countries was not available). We stated this limitation in the Discussion section.

(Page 19, paragraph 2)
"Second, we analysed only 35 countries at most, which limits the number of possible adjustment variables that could be included in the regression analyses."

8. The US is a striking outlier in the cross sectional, but not the longitudinal analysis. I think this merits some discussion – why might this be?

I agree with the reviewer. The US is a clear outliner in cross-sectional analysis. We believe that this may be due to this country's traditional food and agricultural policies and food system that encourage overconsumption. As stated above, cross-sectional analyses did not account for such unmeasured country-specific factors. In contrast, by taking the difference over time within the same country (longitudinal analyses), these factors largely controlled for. We discussed these points in the

Discussion section.

(Page 18, paragraph 2)
"Moreover, we tested the association between social spending and childhood obesity more robustly by examining the association between longitudinal changes as well as cross-sectional relationships. This would have helped to adjust for unobserved country-specific characteristics. For example, the fact that the US was a clear outlier in the cross-sectional analysis might be due to the country's traditional food and agricultural policies that encourages overconsumption.[45] These factors would have been effectively controlled for in the longitudinal analysis but not in the cross-sectional analysis."

Minor points
9. Page 10 lines 3-5
Replace "information" with "variables"
"Furthermore, we examined several country-level sociodemographic information, including the population of children"

Thank you. We revised it (Page 9, paragraph 1).

10. P16 Don't refer to r=0.5 as 'strong' in discussion – it is only moderate
"Social spending on ECEC was strongly associated with reduced obesity growth rates for both girls and boys, while school education was strongly associated with reduced growth of obesity among girls."

In the revised version, we discussed these associations based on the statistical tests for regression analyses. We omitted "strongly" as follows (Page 16, paragraph 3):

"Notably, social spending on ECEC was associated with reduced obesity growth rates for both girls and boys. Moreover, school education was associated with reduced growth of obesity among girls."

Comments by Reviewer 2
Review due for BMJ OPEN: Relationships between Social Spending and Childhood Obesity in High-Income Countries: More Welfare, Less Obesity?
This article analyzes a relevant and interesting research question. The authors provided a notable work in merging several sources of second hand data for OECD countries. Their results show that public investments in education and ECEC are negatively correlated to childhood obesity, cross-sectionally (when absolute values are considered) but also longitudinally (relying on variation rates across time). However, I think that the manuscript can be improved as well as the analytical approach. My comments are below.

Thank you for your review.

Major comments
1. The description of the statistical methodology is unclear and should be rewritten with more efforts for transparency. In line 28-38 page 11 for instance, the authors mention several social characteristics available for each country in the database. Why all these variables are not used as control factors in linear regressions? Indeed, the partial correlation model only controls for 3 variables: GDP per capita, unemployment rate, and poverty rate. Why? Please justify your choice.

The description of the statistical method in the original version has been clarified. Specifically, we instead used multivariable linear regression models which is more popular in epidemiology and more easily understood by the readers, as suggested by the reviewers. In these regression analyses, we adjusted for more variables: the "baseline" prevalence of childhood obesity in 2000, as well as all the social characteristics described in Table 1 (GDP per capita, unemployment rate, poverty rate and the percentage of children aged < 20 years). The findings were qualitatively unaffected after we applied regression analyses. We have clarified these points as follows:

(Page 3, Results in the Abstract section)
"In addition, when we focused on changes from 2000 to 2015, an average annual increase of 100 US dollars in social spending per child was associated with a decrease in childhood obesity by 0.6 percentage points for girls ($p = 0.007$) and 0.7 percentage points for boys ($p = 0.04$) between 2000 and 2015, after adjusting for the potential confounders."

(Page 10, paragraph 3)
"Adjustment variables
We adjusted for countries' demographics and the "baseline" prevalence of childhood obesity in 2000. Demographics consisted of three economic indicators (GDP per capita, unemployment rate and poverty rate) and the percentage of children aged < 20 years, because these factors could affect both the social spending on children [19] and the prevalence of childhood obesity. The "baseline" prevalence of childhood obesity was also included as countries that had suffered from high obesity prevalence in the past may invest more in social programs to mitigate against childhood obesity."

(Page 11, paragraph 3)
"We also examined the association between them, by using a multivariable linear regression model that adjusted for the demographic indicators (GDP per capita, unemployment rate, poverty rate, and percentage of children aged < 20 years) in 2015, and the prevalence of childhood obesity in 2000."

(Page 12, paragraph 1)
"We then investigated the association between them, by using a multivariable linear regression model that adjusted for the changes in demographic indicators (unemployment rate, poverty rate, and percentage of children aged < 20 years) from 2000 to 2015 and the prevalence of childhood obesity in 2000."

(Page 14, paragraph 1)
"After we adjusted for potential confounders (Table 2), we found that countries with higher total social spending in children experienced lower prevalence of childhood obesity ($\beta = -0.3 \times 10^{-3}$; $p = 0.01$ for girls and $\beta = -0.4 \times 10^{-3}$; $p = 0.02$ for boys)."

(Page 14, paragraph 2)
"After we adjusted for potential confounders (Table 3), we found that countries with greater increases in total social spending on children also had smaller increases in the prevalence of childhood obesity ($\beta = -0.6 \times 10^{-2}$; $p = 0.007$ for girls and $\beta = -0.7 \times 10^{-2}$; $p = 0.04$ for boys). These estimates indicate that a 100 US dollars average annual increase (adjusted by PPP and GDP per capita) per child was associated with a decline in childhood obesity between 2000 and 2015 by 0.6% for girls and 0.7% for boys."

2. The introduction of further available characteristics will reduce potential estimation biases due to omitted variables. Specifically, one control factor is of particular concern: baseline rate of childhood obesity for each country. Indeed, omitting this control factor in your estimates, the authors probably understate the protective impact of social spending against childhood weight gain. Indeed, one can assume that a country with a high baseline obesity rate will invest more in anti-obesity policy and

social programs than a country where obesity is low and not a particular issue. Controlling for baseline obesity rates, the estimates might be less influenced by heterogeneous effects, as argued by the authors in line 5-18 page 19.

Please see the answer to the Point 1 above.

3. Line 33 page 19: The authors write that their "findings were based on OECD countries' data and might not be generalizable to low- and middle-income countries". However, in Table 1, several higher-middle income countries are listed as being into the database (e.g. Hungary, Latvia, Mexico, Poland, Slovakia, Slovenia and Turkey). Hence, to investigate potential heterogeneous effects according to the level of a country economic development, I recommend the authors to run additional estimates for specific subsamples (high income OECD countries versus higher-middle income OECD countries). The results might be different and could significantly enrich the existing literature in development studies.

Thank you for bringing us this point. When we divided OECD countries in half according to GDP per capita in 2000 (i.e., countries with lower vs countries with upper GDP per capita) and repeated the regression analyses for each group, we did not find longitudinal association between the changes in social spending and childhood obesity (the coefficients for the association remained negative but did not reach statistical significance). We added the explanation of this post-hoc analysis in the Method and Results sections and amended the discussion point.

(Page 13, paragraph 1)
"Post-hoc analyses
To investigate potential heterogeneous effects according to economic development, we divided OECD countries in half according to GDP per capita in 2000 and repeated the regression analyses for each group (i.e., countries with lower vs countries with upper GDP per capita)."

(Page 15, paragraph 3)
"Post-hoc analyses
In the stratified analyses according to GDP per capita in 2000, we found that the cross-sectional inverse relationship between total social spending on children and prevalence of childhood obesity was observed only in countries with higher GDP per capita ($p=0.03$ for girls and $p=0.04$ for boys) (online supplemental table S2). In longitudinal analyses, the coefficients for the association between the changes in total social spending and changes in the prevalence of childhood obesity remained negative; however, they did not reach statistical significance regardless of the level of GDP per capita and sex (online supplemental table S3)."

Also, considering that OECD countries include higher middle-income countries as well as high-income countries, we made the following two revisions.

-We have changed the title from "Relationships between Social Spending and Childhood Obesity in High-Income Countries: More Welfare, Less Obesity?" to "Relationships between Social Spending and Childhood Obesity in OECD Countries: More Welfare, Less Obesity?"

-We have removed the sentence "Our findings were based on OECD countries' data and might not be generalizable to low- and middle-income countries" (Page 20, paragraph 2) and replaced with

"Although our sample includes both high- and higher middle-income countries, findings were based on OECD countries' data and might not be generalizable to countries outside of this group."

4. Line 50 page 14: Please report the table results for the partial correlation model.

We substituted multivariable linear regression models for partial correlation analyses and showed the coefficients in Table 2 and Table 3.

5. Line 26, page 18: The authors confess in limitations that omitted factors may bias their estimates (e.g. concomitant anti-obesity public policies) and overstate the negative link between social spending and childhood obesity. I suggest to also mention other omitted factors that could understate this negative link, such as baseline variations in childhood obesity rates. Indeed, countries with high obesity prevalence may decide to highly invest in social programs as a reaction and not a prevention. Note that the latter bias can be controlled for by the authors, by including baseline childhood obesity rates in the set of explanatory variables in linear multivariate models.

We agree with the reviewer in that the "baseline" prevalence of childhood obesity is an important confounder in this study. We adjusted for this variable measure in 2000, and the findings were qualitatively unaffected. For further details, please see the answer to the Point 1.

6. Did the authors consider to use multivariate linear regression models, such as a pooled ordinary least square estimator (when cross-section data are used) and time fixed-effect estimator (when longitudinal data are used)? Such estimators are very powerful to estimate relationships controlling for a large set of further characteristics.

As the reviewer suggested, we have used multivariable linear regression models instead of partial correlation analyses (as stated above). When we focused on the change over time in our study, we regressed the absolute change in the prevalence of childhood obesity on the absolute change in social spending on children. The estimates from this analysis are fixed-effects estimators.

Minor comments
7. Line 50-54 page 7: The authors claim that low attention has been paid to the roles of social protection policies in childhood obesity prevention. However, there is a growing literature in development and health economics that analyzes the impact of social protection program and education-based investments on adult obesity (see for instance Levasseur 2019 in World Development; Nagano et al. 2020 in World Development; Etilé 2014 in Economic & Human Biology). The authors could mention these works to complete the literature review.

Thank you for sharing these insights with us. Adding these references will help us better position our research. We cited these papers in the Introduction and Discussion sections as follows:

(Page 6, paragraph 2)
"Although there is a growing literature on the importance of social protection on adult obesity,[15–17] less attention has been paid to the roles of such social protection policies in childhood obesity prevention. This gap partly relates to the difficulty of estimating social spending at the individual level."

(Page 18, paragraph 1)
"There is also a growing literature on the effect of social programs and education on adult obesity.[15–17]"

15 Levasseur P. Can social programs break the vicious cycle between poverty and obesity? Evidence from urban Mexico. World Development 2019;113:143–56. doi:10.1016/j.worlddev.2018.09.003
16 Nagano H, Puppim de Oliveira JA, Barros AK, et al. The 'Heart Kuznets Curve'? Understanding the relations between economic development and cardiac conditions. World Development 2020;132:104953. doi:10.1016/j.worlddev.2020.104953

17 Etile F. Education policies and health inequalities: Evidence from changes in the distribution of Body Mass Index in France, 1981–2003. Economics & Human Biology 2014;13:46–65. doi:10.1016/j.ehb.2013.01.002

8. Line 38-43 page 9: The sentence is unclear. Please reformulate.

Thank you. We revised this sentence (Page 8, paragraph 2).
“Although we anticipate that children will benefit from indirect spending on, for example, unemployment programs and housing, including these categories would overestimate the sums reaching families with children.”

9. Line 47-52 page 9: The sentence is unclear. Please reformulate.

We revised it as follows (page 8, paragraph 2):
“Although education is considered as an essential aspect of social spending,[19] spending on school education is not included in the SOCX datasets (early childhood education and care [ECEC] is included).”

10. Line 36 page 16: The authors may also refer to the results of Anderson and Butcher 2006 (published in Journal of Human Resources), which found that economic pressures for schools are vector of childhood obesity (needing to sell junk food and/or drop gym class to avoid bankruptcy).

This JHR paper is fascinating and highlights the importance of public education investment in preventing childhood obesity. We cited this paper as follows:

(Page 17, paragraph 1)
“Conversely, schools under financial pressures may adopt unhealthy food policy (sales or advertising of snack foods) in schools or cancel gym classes in order to improve the school budget.[37]”

37 Anderson PM, Butcher KF. Reading, writing, and refreshments: Are school finances contributing to children’s obesity? J Human Resources 2006;XLI:467–94. doi:10.3368/jhr.XLI.3.467

11. Line 12 page 17: Regarding gender-based differences in gym class programs, the authors may read and cite Cawley Frisvold and Meyerhoefer 2013, published in Journal of Health Economics.

We cited this paper as follows (Page 17, paragraph 1):
“However, this mechanism may depend on the social context; another study in the US showed that the protective effect of increased physical education on obesity was concentrated among boys because girls substituted physical education for other physical activities.[42]”

42 Cawley J, Frisvold D, Meyerhoefer C. The impact of physical education on obesity among elementary school children. Journal of Health Economics 2013;32:743–55. doi:10.1016/j.jhealeco.2013.04.006

VERSION 2 – REVIEW

REVIEWER	Charlotte Wright University of Glasgow
REVIEW RETURNED	03-Feb-2021

GENERAL COMMENTS	I am very happy with this paper now

REVIEWER	Pierre Levasseur INRAE France
REVIEW RETURNED	01-Feb-2021

GENERAL COMMENTS	The authors convingly reviewed the initial manuscript by taking into account all the reviewers' comments. This article is now acceptable for publication. I have only one remaining minor concern. In Table S2, some fitted coefficients are significant for boys from lower income countries, such as for the maternal and parental leave dimension. Though, page 16, the authors claim "the cross-sectional inverse relationship between total social spending on children and prevalence of childhood obesity was observed only in countries with higher GDP per capita (p=0.03 for girls and p=0.04)." This latter sentence should be nuanced.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 2
The authors convingly reviewed the initial manuscript by taking into account all the reviewers' comments. This article is now acceptable for publication.

I have only one remaining minor concern. In Table S2, some fitted coefficients are significant for boys from lower income countries, such as for the maternal and parental leave dimension. Though, page 16, the authors claim "the cross-sectional inverse relationship between total social spending on children and prevalence of childhood obesity was observed only in countries with higher GDP per capita (p=0.03 for girls and p=0.04)." This latter sentence should be nuanced.

Thank you for your suggestion. We modified the sentence and added information for the stratified analyses of disaggregated social spending as follows (page 15, paragraph 3):
"…, the cross-sectional inverse relationship between total social spending on children and prevalence of childhood obesity was observed among countries with higher GDP per capita (p = 0.03 for girls and p = 0.04 for boys) (online supplemental table S2). When focusing on disaggregated social spending, we found a cross-sectional inverse association between social spending on maternal and parental leave and prevalence of childhood obesity for boys among countries with lower GDP per capita (p = 0.02)."