Author’s response to reviews

Title: ENVIRONMENTAL INJUSTICE AND CHILDHOOD LEAD EXPOSURE IN PERIURBAN (GER) AREAS OF DARKHAN AND ERDENET, MONGOLIA

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RESPONSE TO REVIEWERS’ COMMENTS

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ENVIRONMENTAL INJUSTICE AND CHILDHOOD LEAD EXPOSURE IN PERI-URBAN (GER) AREAS OF DARKHAN AND ERDENET, MONGOLIA

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BMC Public Health

Editor Comments:

The reviewers have raised a number of crucial points which we believe authors must address to improve the manuscript. Points are related both to editorial and technical aspects.
In particular my suggestion is to consider the question related to the choice of LeadCare II, with a high detection limit that provided 42% of values below detection limit. A clarification on this aspect is relevant.

Response: We have stressed that the limit of quantification of LeadCare II was one of the major limitations of the study. We have noted that this made it impossible to examine the exposure-response relationships below this threshold. Out of the 338 children tested, 27.8% had BLL >5 µg/dL implying that the study is not under-powered for the statistical analyses that were performed, however. To temper any potential bias from using only the values above the quantification limit, a median value of 1.65 µg/dL was assigned to any reading below 3.3 µg/dL.

Reviewers made a great work in providing suggestions to improve the clearness of methods used and the way to present results.

Please follow reviewer's suggestions carefully.

Response: We thank the reviewers for the perceptive and useful comments. There is no doubt that the manuscript has benefited significantly from the comments provided by the reviewers.

Reviewer reports:

Sarva Mangala Praveena (Reviewer 1): Introduction

The introduction was writing in chaotic way without a proper flow of the problem statement of this study.

Response: The Introduction has been re-written to better articulate the problem being studied.

A proper discussion on lead exposure and its effect of behavioral problems among children was not presented.

Response: One table (#5) and several paragraphs were devoted to the discussion of the relationships between BLL and behavioral problems (among the children) in the paper. We disagree with the claim that lead exposure and its behavioral problems among children was not presented.

The association between lead and behavioral problems among children was limitedly covered.
Objectives were not clear. A major rewrite is necessary.

Response: The manuscript has been revamped, most of the data analysis has been redone and effort has been made to tighten any loose arguments.

In method focusing on study area, the possible sources of lead to children were limited explained.

Response: The study contained over 30 source-related variables that can influence a child’s blood lead level (key ones are presented in Tables 1 – 3). We disagree with the claim that possible sources of lead to children were limitedly explained

Page 4, Line 41 - How these 338 children were finalized?

Response: We have added the sample size calculation to the methodology section.

Page 4, Line 49 - It was not clearly written why Lead Care II©, (ESA Biosciences) was chosen in this study, or why this instrument was considered compared to ICPMS to detect lead in blood, as most of the studies way of determination.

Response: The advantages of LeadCare II (reliable, inexpensive, and portable which enables the measurement to be made in the field) in relation ICP-MS have been noted in the revised manuscript.

Any QA/QC steps taken in this study.

Response: We adhered to the QA/QC outlined in the User’s Manual of the LeadCare II kit

Bert Morrens (Reviewer 2): Both research topic and study design are highly relevant. The framing of the research results in terms of environmental injustice is certainly justified and illustrates well the expanding scope of the environmental justice literature. However, the statistics and the description of the data contain a lot of imperfections and irregularities, making the manuscript messy and disordered.
Major revisions

1. Due to the high detection limit of the LeadCare II, lead exposure is measured rather inaccurate (42% below detection limit). I therefore wonder if the variability in exposure is adequate enough for doing regression analysis.

Response: We have added this issue as a significant limitation of the study. As we are mostly interested in elevated exposure to lead, we do not believe that the number of children with BLL <3 ug/dL has affected our main conclusions.

Furthermore, there seems to be a warning from the CDC about risks of inaccurate results from lead tests with the LeadCare II in the same period as the study sampling (may 2017): https://www.cdc.gov/nceh/lead/about/blood_lead_test_safety_alert.html

The authors should clarify this point.

Response: We are aware of the recent FDA warning. The warning was based on data showing that test results on blood drawn from a vein may be lower than the actual level of lead in the blood. This warning, however, did not pertain to LeadCare Blood Lead Testing Systems when used with finger or heel prick (capillary) blood samples – as was the case in this study (see https://www.accessdata.fda.gov/cdrh_docs/reviews/K142705.pdf)

2. There is a lack of specificity regarding the methods, particularly the operationalization of variables and the description of the data analysis in the tables.

Response: We have provided the information on the recruitment process and key variables assessed with the survey instrument

- Table 1: no reference to the log-transformation of the data; not clear if the p value belongs to mean value or to the % < >5 µg/dl.

Response: We have redone the tables using log transformed values of BLL, the outcome measure in the models. We decided not to dichotomize the BLL values in the data analysis. Instead, we ran bivariate analyses of the relationships between BLL (a continuous variable) against the subjective data for categorical variables obtained with the survey instrument. We used the Spearman's rank correlation coefficient (instead of the Pearson’s correlation coefficient to assess the relationship between any two variables in the revised manuscript.)
- Table 2/3: these tables are difficult to interpret since there is no indication of the data type (continuous or categorical variables?); replace 'sig. (2-tailed)' by 'p-value'; the asterisks behind the Pearson correlations are unnecessary addition when p-values are also given.

Response: We have changed Pearson correlation to Spearman’s correlation coefficients. We have replaced the “sig. (2-tailed)” with “p-values”. The asterisks have been removed from the two tables.

- Table 4: the title of this table is not clear (only child risk factors and no environmental factors were considered?); the coefficients are difficult to interpret without information about the number of categories for each parameter. The description of the final equation on p. 9 line 53-54 is unclear and in my opinion not correct.

Response: Agree. The table has been completely redone. The equation has been deleted from the revised manuscript.

- Table 5: this table cannot be interpreted. This should be the summary of a multiple regression model (as stated in the text), but the title mentions ‘correlations’ and the table shows no models or coefficients (‘F’ and ‘Sigma’ are not clear).

Response: Table 5 has been revised. The title now says the results of linear regression models, and coefficients are provided in the table.

3. Throughout the manuscript, the terminology of various concepts and variables is used inconsistently, making it difficult to follow the storyline and interpret the tables. I suggest the authors to bring more uniformity in the terminology used.

Response: We have made concerted effort to use the same names in the text and the tables in the revised manuscript.

- ‘Occupation’ - ‘parents have a job’ - ‘parents holding a paying job’ - ‘socio-economic status’. Response: “Parents holding a paying job” has been replaced with “parents have a job” throughout the manuscript. Occupation has been dropped as a variable in the data analysis as there was lack of clarity between occupation and having a job among the study participants.
- 'Parental educational level' - 'parents highest degree'. Response: We have changed “parents highest degree” to “parental education level” throughout the manuscript.

- Sex (table 1) - gender (table 2). Response: We have replaced “sex” with “gender” throughout.

- 'adults' - 'parents' - 'respondent'. Response: We have changed “adults risk behavior” to “parental risk behavior” throughout. We have replaced “respondent” with “parent”.

- 'type of house' (table 3) - 'apartment type' (p.8, line 46): Response: There are different types of houses and apartment buildings in the study area, and we are reluctant to replace one with the other.

- 'lead exposure' - 'lead poisoning'. Response: Since lead exposure is not synonymous with lead poisoning, we have made sure that the two terms are used in proper context in the paper.

4. Some statements in the text appear to be incorrect.

- P.5 line 42 the 'less-than' symbol is displayed wrong (BLL < 5 µg/dL) [Corrected]

- p.7 line 56-57: the variable 'parental work in an environment with lead' is not related to BLL according to table 2 (variable 'lead related job or hobbies' p=0.536). Presumably, the authors talk about the variable 'occupation' in table 2 but the relation with lead environment then is not clear. [Good point; the text has been changed to correct the discrepancy between the text and the data in Table 2]

- P.8 line 50: the variable 'peeling paint off the wall' does not appear in the table. [Mention of this variable has been deleted from the text]
- Table 3: numbers are missing in block Heating method (line 16) [Values for N have been added]

- P.10 line 28-29: the difference between residential structure (ger tent versus apartment building- and residential location (tent city versus city proper) is not clear throughout the rest of the text. For instance in table 1 the term ’house type’ is used for apartment versus ger area. [ For consistency purposes, we have replace “residential structure” with “house type” and “apartment building” with “apartment”]

- p.10 line 43: F=3.91 instead of F=0.3.91? [Oops; should be 3.91]

- p.12 line 16 stated that 58% had BBL >5µg/dL but on p.6 line 10 it is 27.8% [The error has been corrected; the number should be 27.9%]

- p.13 line 11-13: sentence starting with ’Average blood’ is grammatically incorrect. [Missing brackets have been inserted]

- p.13 line 32: the statement that there is an increased risk of behavioural problems among children in ger households is not supported by the regression models in table 5. Ger vs city proper showed no significant correlations with behavioural problems. Strongest correlations are for parental occupation.[The statement has been revised to say that it is the disparities in lead exposure that have likely mediated the increased risk of behavioral problems]

Minor revisions

5. Some statements and sections need more elaborating or clarification.

- P.2 line 16: 5 µg/dl should be referred to as the US CDC reference value and not a level of concern. [Correction is made]
- P.4 line 10-11: the authors should briefly explain the EJ framework with some key references. [Done]

- p.5 line 8-10: more emphasis on the operationalization of some variables is needed in order to correctly interpret tables 2-4, for instance are the variables dichotomous, categorical or continuous? [Key variables have been mentioned, as suggested]

- P. 6 line 11-27: when results are compared with other national and international studies, it would be useful to indicate whether these studies used the same sampling method (with a high LOD) or a more accurate one (venous blood sample). Also, the children in the Ulaanbaatar study were aged 7 to 14, while children in this study were aged 4 to 7. Since BLL can increase with age, this should be mentioned when comparing studies. [Good suggestions. The age of children monitored in UB, and the difference in analytical methods used in the two studies in UB have been noted in the revised manuscript].

- P.10 line 23: I suggest replacing the term 'income disparities' with 'socio-economic status' because this is also used on p. 11 line 5. [The change has been made].

- p.10 line 56: not clear what 'externalizing issues' are. [Examples of the externalizing issues have been added]

- P. 11 line 8-9: the Belgian study had a different study design: it compared prenatal lead exposure with behavioural problems at age 7-8 years. [The difference in age has been mentioned in the revised manuscript]

- Table 2: not clear what 'respondent' mean on line 35 and how this differ from 'adults' on line 26. [“Respondent” has been changed to “Parent” as suggested]

- Table 2: not clear how the variable 'Who is smoking' should be interpreted. [Variable has been revised to “Any smoker in the home”]
- Table 2: I suggest adding to the title 'social' risk factors of lead exposure, to make the
distinction with the 'environmental' risk factors in table 3. [Excellent suggestion]

Diogo Francisco Santos Silva Pestana (Reviewer 3): The paper titled 'ENVIRONMENTAL
INJUSTICE AND CHILDHOOD LEAD EXPOSURE IN PERIURBAN (GER) AREAS OF
DARKHAN AND ERDENET, MONGOLIA", provides a nice and compared examination of
lead exposure between children in the ger districts and the city proper.

The study shows that children in the ger communities of the urban areas of Mongolia are socio-
economically disadvantaged and disproportionately exposed to lead, leading to increased risk of
behavioral problems among these children, even at BLL <5 μg/dL previously defined as the level
of concern. So, it reaffirms the notion that there is no safe blood lead concentration in children.

I think it is an interesting paper, addressing the very important topic of environmental injustice,
well written and with some novelty. I believe that the paper could provide an important
contribution of the topic dealt, however I also make some comments which can be considered by
authors to improve the quality of the paper.

Response: We are glad that the reviewer thinks that our paper is interesting.

Comment 1: In Methods, Statistical analysis section (page 5), the authors describe the tests used,
which are parametric. However, it is unclear whether the normal distribution was verified. Since
this is often not the case for exposure to contaminants, this should be clarified.

Response: We have redone the data analyses using log-transformed values of BLL and BMI
which are not normally distributed. The other continuous variable (age of children) was
normally distributed (skewness = 0.064) and needed no log transformation. The risk factors and
other socio-demographic co-factors were categorical variables, and log normalization was not
applied.

Comment 2: Following the previous comment, I believe the Table captions should be more
detailed, namely because it is not completely clear in the tables and their respective captions
what tests are used and what is being compared.
Response: Table captions have been revised and notes added at the bottom of the tables about variable properties.

Comment 3: In page 5, line 39 the authors say "In order to explore the effect of the various explanatory variables on BLL, children were divided into 2 groups: those with BLL ≥5μg/dL and BLL>5μg/dL.".

Should not it be "BLL ≥5μg/dL and BLL<5μg/dL"? This should be corrected.
Response: Correction has been made; analysis with dichotomized BLL data is no longer included in the revised manuscript and the sentence has hence been deleted.

However, it is not clear throughout the description of the results and tables where there is this division into two groups. Is it in the results presented in Tables 2 and 3? This should be clarified.
Response: Model analysis based on dichotomized BLL values was not included in the paper and the sentence has been removed.

Comment 4: Following the previous comment, Tables 2 and 3 indicate that Pearson's correlations were applied, but from what I've come to realize, in this work, this analysis seems to be done between categorical variables and not between continuous variables. Therefore, Pearson's correlation should not be used. Is there any missing information in the description and in the tables? This should be clarified and corrected.
Response: Good suggestion. We have redone the regression models and have used the Spearman’s rank correlation coefficients in the revised manuscript to describe the relationships.

Comment 5: In page 5, line 42 the authors say "One hundred forty one (41.7% of total) had BLL<3.3μg/dL detection limit. For these cases, a value of 1.65μg/dL was used in statistical calculations.". What was the rationale behind the choice of 1.65μg/dL for all values below the detection limit. Since the percentage of values below this limit is high, data analysis may be affected. For this reason, authors should explain and justify this option.
Response: 1.65 ug/dL is the median value between 0 and 3.3. This is a fairly common way of dealing with data below the quantification limit. We have mentioned the high detection limit as a significant shortcoming of the study.
Comment 6: The authors report that there are differences in lead exposure between children in the ger districts and the city (Table 1), but explore this relationship only considering the two cities together. Since the cities had different characteristics and even had different exposure levels (Table 1), would the comparisons between ger districts and city be similar between the two cities? It would be interesting to present some reference to this evaluation.

Response: Good comment. We attributed the difference in average BLL of children in Erdenet and Darkhan to the effect of lead pollution from the large mining operations in the former city. The revised manuscript includes the statement that since the genesis and living conditions in the ger districts of the two cities are similar, we decided not to treat them as separate entities in the data analysis.

Comment 7: In page 7, line 56 the authors say "Parental education and parental work in an environment with lead were significantly correlated with children's BLL". Some of the risk factors are not well described, namely "work in an environment with lead" where the corresponding parameter was "Occupation". They should be clarified.

Response: We have changed “parental work in an environment with lead” to “lead related job or hobbies” which was the term used in the questionnaire. Occupation was no longer included as a separate risk factor in the re-analysis of the data.

Comment 8: In page 10, line 19 the authors say "Our results are consistent with those of Enkhbat et al [16]) who did not find any significant difference between the BLL of women who live in ger tents versus the city dwellers in UB.". Isn't this statement contradicting the results of the study that show that there are differences in lead exposure between children in the ger districts and the city?

Response: We agree that the sentences are confusing. The section has been re-written to make it clear that we did not find any significant correlation between the burning of coal and BLL of children in Erdenet and Darkhan and that Enkhbat et al also did not find any correlation between the BLL and burning of coal in their study of women who live in ger tents versus the city dwellers in Ulaanbaatar. The sentence pertains to one specific parameter – burning of coal – which does not appear to be an important effector of blood lead as people had previously thought.

Comment 9: In page 10, line 46 the authors say "(Table 5). These effects were observed at the low average BLL (< 5 μg/dL) of the children that participated in this study.". As mentioned in previous comments, it is not explicit how this assessment was carried out. I assume that a
division into two BLL groups was made, but this is not explicit in Table 5. Descriptions should be improved to make interpretation easier.

Response: The low average BLL of the children studied was 3.85±2.58 µg/dL. The sentence has therefore been revised to “These effects were observed at the low average BLL (3.8 µg/dL) of the children that participated in this study”.

Comment 10: In page 13, line 19 the authors say "This study contributes further evidence showing that BLL <10 µg/dL, previously defined by the CDC [25] as the level of concern, is a threat to children's health.". Should not it be "BLL<5µg/dL"? This should be corrected.

Response: Correction made. Sentence has been revised to “This study contributes further evidence showing that BLL as low as 3.8 µg/dL can be a threat to children’s health”