Author’s response to reviews

Title: Association between ambient particulate matter concentration and fetal growth restriction stratified by maternal employment

Authors:

Seung-Ah Choe (seungah9932@gmail.com)
Jiyeong Jang (jiyeongj@gmail.com)
Min Jung Kim (mjkim.stat@gmail.com)
Yoon-Bae Jun (junpeea@snu.ac.kr)
Sun-Young Kim (sykim@ncc.re.kr)

Version: 2 Date: 22 May 2019

Author’s response to reviews:

Dear Dr. Yeyi Zhu,

We are resubmitting our revised manuscript entitled “Association between ambient particulate matter concentration and fetal growth restriction stratified by maternal employment”. We revised our manuscript taking into account the suggestions of reviewers. Our responses to each of the comments from the reviewers were enclosed.

Thank you for your positive consideration of our manuscript.

Sincerely,

Sun-Young Kim, PhD
Department of Cancer Control and Population Health
Graduate School of Cancer Science and Policy, National Cancer Center
Reviewer reports:

Hualiang Lin (Reviewer 1): The authors have addressed the questions well. I would suggest to accept.

Xin Cui, Ph.D., MPH (Reviewer 2):

Comment 1: In the abstract, the authors mentioned "Among 824,011 singleton term births, 44.0% (279,856) were employed and 66.0% (544,155) were non-employed mothers". Numbers were revised by the authors but 44.0% + 66.0% is still greater than 100%. I suppose that there is a typo, i.e. 44.0% should be replaced by 34.0%. Please clarify.

Response 1: Thank you for the comment. We corrected our mistake (line 52 in the revised manuscript).

Comment 2: In the results the authors added "Maternal employment was associated with exposure (P = 0.040 for entire pregnancy PM10 and -0.049, P < 0.001 for entire pregnancy PM2.5) and outcome (P < 0.001 for SGA and -0.091, P < 0.001 for LBW) with adjustment for all covariates.". I wonder whether -0.049 and -0.091 are the correlation coefficients or effect estimates from regression models, and why such number was only provided for PM2.5 instead of PM10 (same for the outcome, i.e. number was provided only for LBW but not for SGA).

Response 2: We clarified the numbers presented are the regression coefficients of exposure (PM10 and PM2.5) or outcome (LBW and SGA) on maternal employment (line 232-235).
Comment 3: The authors added a DAG and mentioned "We used directed acyclic graphs (DAGs) analysis to examine potential confounders and mediators in the association between air pollution (Supplemental Fig. 1).". First of all, the sentence seems to be incomplete, i.e. the association between air pollution and what? Also, employment is a potential confounder in this causal diagram instead of a mediator. Mediator has to have the relationship of Exposure $\rightarrow$ Mediator $\rightarrow$ Outcome, i.e. an arrow needs to point from the exposure to the mediator, which is not the case here.

Response 3: We corrected the “mediator” to “effect modifier” in the revised manuscript to avoid confusion. In effect modification, the direction of arrow can indicate the distribution of exposure (air pollution) conditional on maternal employment.

Reference:

Weinberg CR. Can DAGs clarify effect modification?. Epidemiology. 2007;18(5):569–572. doi:10.1097/EDE.0b013e318126c11d

Chenxiao Ling, Ph.D (Reviewer 3):

The authors have sufficiently addressed most of the initial comments. Below are a few further comments.

# Comment 1 (regarding previous comment 8):

The authors explained that "..., these sites would not represent residential exposure" and "that these urban background monitoring sites only would better represent the level of air pollution exposure for people". Since the title and the abstract only suggested "outdoor particulate matter", one would assume that it also includes traffic-related air pollution, if not mentioned as "ambient" air pollution. I'm with reviewer 2 on the comment 7 that the authors should consider changing the title. Please note that maternal residential addresses could be used to assess traffic-related PM or non-traffic-related PM (the latter seems to be your main focus), even if occupational exposure is not the exposure of interest; therefore, it is important to make the distinction.

Response 1: We assumed that outdoor air pollution can be considered as ambient air pollution (WHO 2018). We excluded urban roadside sites to assess residential exposure including traffic-
related air pollution rather than air pollution directly emitted from traffic sources of major roadways. The potential health effect of traffic-related air pollution was found within relatively large distances of several hundred meters or a few kilometers (HEI 2010; Yi et al. 2017). To avoid confusion, we revised the title and sentence (title and line 154-155 in the revised manuscript)

Reference:


HEI Special Report 17.


Comment 1-2: The phase "exposure for people" (page 8 line 40) seems somewhat ambiguous, please reword (e.g., for people who…).

Response 1-2: We revised to ‘for people who were living in the district.’ (line 158).

Comment 1-3: How many gus have more than one monitor station and how many have none? Please state how these situations were handled and how estimates were derived in the "PM data and exposure assessment" section (e.g., averaged for >=1 stations and excluded without a station?).

Response 1-3: Only two gu’s had a second monitoring site for a short time; these sites were excluded from the analysis (line 162-163).
Comment 1-4: When the authors excluded those monitoring stations near heavy traffic, did they also exclude mothers living in proximity to traffic as well? If so, please state that in your methods. If not, is it possible that a mother would seem to have low exposure level only because a station with high exposure level is near heavy traffic and excluded? For example, if a mother lives in a gu with a “in-community” station with relatively low exposure and another station near traffic with high exposure but excluded?

Response 1-4: Although it will be interesting to look at the health effect of mothers according to the distance from major roadway, it was not feasible in our study as mothers’ addresses were available only at the gu level. All the mothers living in the same district were assumed to be exposed to the same amount of air pollution. The potential measurement error might have contributed to the null findings. We addressed this point in the Discussion (line 379-384).

Comment 1-5: It might be helpful to create a map as a supplementary material with showing the gus, the major roads/highways, and the included (n=25) and excluded (n=15) monitoring stations?

Response 1-5: We added a map as supplemental Figure 1 (Supplemental Figure 1) to clarify the locations of urban roadside monitoring sites next to the major highways.

Comment 1-6: I suggest that the authors at least do a sensitivity analysis not excluding monitoring stations near traffic and see if the main associations change.

Response 1-6: Previous simulation studies showed that including source-related monitoring sites such as roadside sites without incorporating those information to building exposure models provides biased health effect estimates (Lee et al 2015; Szipro and Paciorek 2013). As we did not use modeled exposure, we believe that our choice of excluding roadside sites for our health analysis helped avoid possible bias which would have occurred otherwise.

Reference


# Comment 2 (regarding previous comment 4):

The ongoing trend in epidemiology discourages the misinterpretation of p-value. It is one of the common mistakes to dichotomized it using 0.05 as the cutoff (please see below reference). I suggest that the authors refrain from using p-values this way as well as not bold the estimates with $P < 0.05$. Please modify the description of findings in the manuscript accordingly.


Response 2: We agree that we should not rely on P values as a sole criterion for conclusion. We removed the bolded estimates in the tables (Table 2 & 3). However, we decided to keep P values in the text, because other reviewers suggested to present P values as additional information to complement our narrow 95% CI estimates in the first review.

Tao Liu (Reviewer 4): All the questions have been answered with satisfaction, and I suggest to accept it for publication.

Samantha Kingsley (Reviewer 5): All of my comments were adequately addressed.