Author’s response to reviews

Title: A Global Perspective on Coal-fired Power Plants and Burden of Lung Cancer

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Author’s response to reviews:

Reviewer #1: 19 model, we estimate a total of 1.41 million standard
Comment: Please give an uncertainty range.

Response: Thank you for your suggestion. We provided the uncertainty ranges in the abstract, Results, Supplementary Table 5 accordingly.

In abstract, we modified the sentence, reading: “Based on the model, we estimate a total of 1.37 (range=1.34 ~1.40) million standardized incident cases from lung cancer were associated with coal-fired power plants in 2025.” (new page 2, new paragraph 3, new line 19-20)

In Result, we added “Supplementary Table 5 presents the PAFs and standardized lung cancer cases attributable to coal-fired power plants among males and females, respectively, in 2015 and 2025. PAFs are higher for females than males in most countries due to higher RRs. Australia (39.26%) and US (32.65%) had the highest PAFs in 2015, corresponding to 10,308 and 233,647 standardized lung cancer among females, respectively. In China, we estimated 347,565 (range=340,592~354539) standardized lung cancer among females (PAF=19%) and 786,247
(range=769.295~803199) among males (PAF=15%) in 2025, based on different fertility scenarios estimated from UN.” (new page 10, new paragraph 3, new line 16-22)

Significant digits in Table 1.

No more than two or three digits are justified based on the variation in values, not just two decimal places.

Thus, 454.07 (60.55~942.45) should be 450 (60-940),
Males 32.23 (12.36~54.12) should be 32 (12-54).

Please adjust in the table and in the text.

Response: Thank you for your suggestion. We modify the manuscript and Table 1 accordingly.

In result, the revised manuscript reads: “From the first period to the last, average age-standardized incidence rates from lung cancer decreased by 46 (i.e., from 454 to 408) per hundred thousand (10%) in males but increased by 12 (i.e., from 143 to 155) per hundred thousand (8%) in females. Coal capacity increased from 16 GW to 23 GW. Smoking prevalence decreased by 9% in males and 11% in females, respectively.” (new page 8, new paragraph 2, new line 15-21)

Reviewer #2: The Authors have not acknowledged my opinion about the risk of ecological bias in their results. On the contrary, they report in the answer to my comments that ecological fallacy is unlikely, because the three criteria highlighted in the pivotal paper by Robinson in 1950 are not fulfilled. In particular (if I have rightly interpreted the Authors point of view), ecological fallacy should be unlikely because:

a) results are not intended to be applied at an individual level;

b) results are consistent with similar findings reported in the scientific literature.

With regard to the first point, I agree with the Authors that one of the major merit of their investigation is that the results can be used to address key policy questions, but this statement
implies that estimates obtained at an ecological level should be (at least approximately) similar to those existing at the individual one.

With regard to the second issue, I also agree with the Authors that consistency with findings from independent studies supports their results. However, even if a reverse bias from ecological fallacy is very unlikely, other sources of over- or under-estimation related to the ecological design cannot be ruled out.

The manuscript has improved a lot after the revision. However, I suggest few minor revisions before its definitive acceptance.

Response: Thank you for your suggestion. We are very appreciated to have a chance to examine the possible ecological bias through the in-depth discussion. The conversation makes our understandings of possible bias more clearly. We are also very appreciated that with mutual agreement that ecological bias is unlikely due to two reasons mentioned above, you also kindly reminded us the over- or under-estimations issues related to the ecological designs.

For the point 1, we listed the similar results at the individual levels in the introductions and discussions. For examples, we also mentioned “Both adenocarcinoma (45) and squamous cell carcinoma (46, 47) of lung might have association with environmental factors” in discussion. We also mentioned “Most available estimates of health risk associated with electricity generation are oversimplified since they are calculated by multiplying a factor to air pollution levels (either PM2.5 or PM10) without considering the heterogeneous compositions of particles from different sources (13-15).” in introduction.

For the point 2, we agree with your comments that other sources of over- or under-estimation related to the ecological design cannot be ruled out. We added the following sentence in limitation, reading: “Other factors that may lead to overestimation or underestimation related to the ecological design should also be considered hereafter.” (new page 13, new paragraph 1, new line 4-5)

1) Study limitations section, page 13, row 4: The sentence "the potential for 'ecological fallacy' is unlikely … " should be rephrased using less emphasis (e.g., "a strong impact of ecologic bias is
unlikely”) and without referring to the fact that estimates are not intended to be used at individual level.

Response: Thank you for your suggestion. We deleted the referral to individual level and modified the sentence accordingly, reading: “Despite using an ecological study design, a strong impact of ecologic bias is unlikely (41) because our analysis on aggregated data is meant to infer policy decisions at the national level and for international comparison (42).” (new page 13, new paragraph 1, new line 2)

2) Table 1. Instead of reporting 95%CI the Authors decided to show the 2.5th and 97.5th quantiles. It sounds good, but in the footnotes 95%CI are still mentioned. Furthermore, in my opinion, mean values should be replaced by median ones, which are more suitable to describe non-Gaussian distributions.

The selected centiles (2.5 and 97.5) are meaningful in that they provide an estimate of the entire range of values for each variable avoiding the effect of outliers. However, IQR is more frequently used in scientific literature, then I would suggest that the Authors add a small sentence to better justify their choice.

Response: Thank you for your suggestion. We erased the note under table 1 due to redundancy. However, the range from 2.5 percentile and 97.5 percentile is exactly the 95% confidence interval, without any assumption of distribution. Since Table 1 is just a summary table of the data itself, not an analysis, therefore, either mean and 95% CI or median and IQR should be fine. We would like to add the following sentence to justify our choice, reading: “Table 1 displays the mean and 95% intervals of all covariates during the three periods of 2000~2004, 2005~2010 and 2011~2016; note that these summaries are averaged over countries and time; obtained from empirical data without any distribution assumptions.” (new page 8, new paragraph 2, new line 15-17)

3) Page 14, row 5: "we can obtained" should be changed to "we can obtain".
Response: Thank you for your suggestion. We modified accordingly, reading: “GBD study is the best available data we can obtain” (new page 14, new paragraph 1, new line 3)