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: 15 Table 1 displays the mean and 95% intervals
should be
15 Table 1 displays the mean and 95% confidence intervals
Response: Thank you. We corrected accordingly. (New page 8, new line 15)

Thanks for responding to my comments.

However, this statement still has too many non-zero digits we estimated 347,565
(range=340,592~354,539)
21 standardized lung cancer among females (PAF=19%) and 786,247 (range=769,295~803,199)
The range indicates an uncertainty of 2 percent.
Thus, they should have two or at very most three non-zero digits.
Response: Thank you. We corrected accordingly as following: “corresponding to more than ten thousands and 233 thousands standardized lung cancer among females, respectively. In China, we estimated more than 347 thousand (range=341,000~355,000) standardized lung cancer among females (PAF=19%) and 786,000 (range=769,000~803,000) among males (PAF=15%) in 2025, based on different fertility scenarios estimated from UN.”. (New page 10, new line 19 to new page 11, new line 2)

Reviewer #2: The revised version of the manuscript entitled "A Global Perspective on Coal-fired Power Plants and Burden of Lung Cancer" has further improved and in my opinion it is now acceptable for publication in Environmental Health.

I just suggest that the Authors provide some very minor changes without the need of any further revision:

1) Abstract (page 2, row 20): "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".

Referring to a forecast measure, "were associated" should be replaced with "will be associated".

Response: Thanks for your careful reading. We modified accordingly, reading: “Based on the model, we estimate a total of 1.37 (range=1.34 ~1.40) million standardized incident cases from lung cancer will be associated with coal-fired power plants in 2025.” (New page 2, new line 18-20)


Response: Thanks again. We revised as “Lung cancer codes were B101 or 162 in International Classification of Diseases version 9 (ICD-9); C028, 162, 231.1, or 231.2 in ICD-9CM” (New page 4, new line 15)
3) Results (page 8, row 17): "note that these summaries are averaged over countries and time; obtained from empirical data without any distribution assumptions"

This sentence should be moved to the Data Analysis section.

I am not completely convinced that percentiles selected from a distribution from a set of observed values (especially from different populations) can adequately fit the definition of confidence interval. However, I don't want to insist on quibbles that do not affect the interpretation of the interesting results of the study.

Response: Thank you for your suggestion. Reviewer 1 suggested us to put 95% confidence intervals back to describe the observed values and we just simply reminded the readers to prevent any confusing.

4) Results (page 9, row 20): "Univariate, behavior-environmental […] models were applied".

"Univariable model" should be preferred to "univariate model" when referred to a regression analysis.

Response: Thank you for your suggestion. We modified accordingly. (New page 9, new line 20)

5) Results (page 10, row 20): "340,592~354539" should be changed to "340,592~354,539"

Response: Thank you for your carefully editing. We modified accordingly. Reviewer 1 has additional suggestion on significant digit and we revised as the followings, reading““corresponding to more than ten thousands and 233 thousands standardized lung cancer among females, respectively. In China, we estimated more than 347 thousand (range=341,000~355,000) standardized lung cancer among females (PAF=19%) and 786,000 (range=769,000~803,000) among males (PAF=15%) in 2025, based on different fertility scenarios estimated from UN.” (New page 10, new line 19 to new page 11, new line 2)

6) Study Limitations (page 13, row 1): "Despite using an ecological study design, a strong impact of ecologic bias is unlikely because our analysis on aggregated data is meant to infer policy decisions at the national level".
In my opinion ecological fallacy is independent from the future application of the study results. I suggest the Authors rephrase the sentence as follows:

"Despite using an ecological study design, biological plausibility of our results, the lack of any association in the falsification analysis, and the consistence of our estimates with those from previous investigations indicate that a strong impact of ecologic bias is very unlikely. Moreover, our analysis on aggregated data is meant to infer policy decisions at the national level …"

Response: Thank you for your suggestion. We modified accordingly, reading: “Despite using an ecological study design, biological plausibility of our results, the lack of any association in the falsification analysis, and the consistence of our estimates with those from previous investigations indicate that a strong impact of ecologic bias is very unlikely. (41) Moreover, our analysis on aggregated data is meant to infer policy decisions at the national level and for international comparison (42).” (New page 13, new line 4-7)