Review of "Different response characteristics of ambient hazardous trace metals and health impacts to global emission reduction"

Summary

The manuscript "Different response characteristics of ambient hazardous trace metals and health impacts to global emission reduction" presents a model-based estimate of the 2020 minus 2019 health impacts of various trace elements, such as lead and arsenic. The authors develop emission inventories of these trace elements for 2019 and 2020, simulate their transport in a global chemical transport model, and estimate the health benefits due to atmospheric trace element concentration reduction due to the COVID-19 pandemic. This manuscript presents model-based evidence on the wide-ranging side benefits of emission reductions, especially in regions with already higher emission levels of toxic trace elements. The main findings of this work are that lead and arsenic reductions may have caused the largest health benefits among the trace elements considered, and sources such as coal combustion and smelting may have contributed the largest to their emissions reduction during the 3 months of the pandemic. However, the presentation in the manuscript could be highly improved. The methodology and its assumptions were not clear. The authors use only 3 months of data from 2019 and 2020 to estimate health effects. A short period like this may not be representative of long-term health effects and trends. Inventory, model, and health effect calculations were not presented clearly. Overall, I believe the manuscript's findings are useful in highlighting the positive impacts of trace element emissions reductions in different parts of the world, and I recommend the manuscript for publication after major revisions.

High-level Comments

1. Tighten the Introduction. The gaps and motivation for the design of the study were unclear. "What motivated you to perform this work the way you did?"
2. Consider writing an overview of the Methods, highlighting its flow.
3. Sec. 2.1 Inventory development was unclear. Consider tabulating it in the manuscript or supplementary information, including values from Zhu et al., used in this work. Consider describing the spatio-temporal resolution of the inventory, exact sources/sectors, and emission factors.

4. Sec. 2.2. Consider adding a table of the observations segregated by macro regions, species, number of observations, and time-period of observations.
5. It was unclear why if 2020 emissions were used, why low-emission scenarios were performed? Moreover, findings from the 20-80% reduction scenarios did not seem central to the paper. The basis for the reduction scenarios and amounts was not clear.
6. Comparing 2020 emissions against only 2019 meteorology may not provide a robust assessment of emissions-driven changes. This is because 2019 could be meteorologically anomalous, which could bias particle suspension and transport, even if an additional sensitivity simulation using 2020 emissions with 2019 winds is included. To better isolate the effects of emissions from meteorology, I recommend the following approach: Run one set of simulations using 2020 emissions and 2015–2019 MERRA-2 winds, and another set using 2015–2019 emissions with the same 2015–2019 meteorology. Comparing the monthly averages from these two experiments would minimize the influence of any single-year meteorological anomaly and more clearly attribute differences to emissions changes. If computer time is a limitation, I recommend performing at least 3 years of simulations for robustness.
7. The analysis focus in the Results section was not consistent – it fluctuated from micro-regions in one country to global macro regions in the same paragraphs.
8. Sensitivity analysis is required to derive robust conclusions for health impacts. Consider compiling a broader set of risk values from the literature and using a lower and upper bound for the calculation. Then, a Monte Carlo-like sensitivity simulation could yield uncertainty bounds to health impacts. If computationally limited, I suggest using absolute lows and highs to obtain the lower and upper bounds.
9. Overall writing could be tightened.

Specific Comments

L96: Detailed data of what? Please clarify.

L104: Refer to it as “Text S1”.

L114: Add exact reference and the table (or file) of the compiled observed dataset.

L59, 115, and others: Add the full form of the model names at least once.

L126: Are the natural sources different in 2019 and 2020 in the model? Clarify.

L127: Define “TE”.

L164: Why is EF 365 days if the assessment is done only for a few months?

L178: Check superscript formatting (compared to L193).

L183-185: Explain this in detail. Why would simulating only one region make it underestimate the concentrations?

L191: Shorten the section heading.

L201: It is unclear which exact regions are being analyzed in this manuscript. This can be clarified using a table or listing out the regions up front the methods.

L228: It contradicts the previous statement. Most residential energy does not include coal combustion when globally averaged.

L261: The treatment of trace elements in the model should be clarified in the methods.

L287: Why aggravation? Clarify.

L288-290: If so, I recommend performing health analyses for each of these regions to confirm this hypothesis. You can separate emissions and meteorology impacts using the framework already used in the manuscript.

L293: It should be up front.

L313: Probably better to show emissions from individual sectors considered in this work and their 2020/2019 ratios for some representative regions.

L322: This contradicts L313, which states that Pb increased globally, even if slightly. Please clarify.

Figure 1: What does the colorbar indicate? If there is no variability (all blue color), consider changing the colorbar scale to highlight any variability.

Figure 3: What do the violin plots show?

Figure 5: Why is there inter-regional disparity in CR and HQ risks in China and India? I understand it is described in the manuscript but more clarification is needed (see above comment).