

The authors coupled the HTAP_v2.2 global air pollutant emission inventory with the global source receptor model TM5-FASST to evaluate the relative contribution of the major anthropogenic emission sources to air quality and health in 2010. As I noted in my previous review, I find that what the paper is trying to do is important. However, I still find that the objective of the paper is unclear throughout the paper and I am not sure if this paper should be stand-alone or should be combined together with the Van Dingenen et al. (2018) paper that is currently under review for ACP. Most importantly, I do not understand the rationale behind quantifying health impacts from sectorial emissions, given that the uncertainty is so high.

First, the biggest problem I have with this paper is that there are significant underestimations of PM_{2.5} concentrations in many countries and to me, the linearity estimation for PM_{2.5} is not satisfactory. I am not convinced that there is new science in the paper and as one of the reviewers was suggesting, maybe this paper, combined with the Van Dingenen et al. (2018) paper should probably be moved to GMD to discuss potential of the new tool for assessing air quality and health impacts.

Second, the objective needs to be better defined. As for the two objectives of the study, there are two sentences in the manuscript:

P. 2, l. 46 “The objective of this study is to evaluate the relevance of uncertainties in regional sectorial emission inventories, and their propagation in modelled PM_{2.5} concentrations and associated impacts on health.”

P. 3, l. 15 “A second objective of this analysis is to evaluate the importance of emission uncertainties at sector and regional level on PM_{2.5}, to better inform local, regional and hemispheric air quality policy makers on the potential impacts of sectors with larger uncertainties or regions.”

The two are very similar and I am not sure if the second objective is necessary. On p. 4, l. 12, the aim of this work is explained “to address the uncertainty of sector specific emissions from this inventory in a quantitative way as well as the differences we observe from one region to the other, based on the uncertainty of activity data and emission factors.” Furthermore, later in the text on p. 13 l. 38, the authors state, “[i]n our work we only evaluate how the uncertainty of emission inventories influences the health impact estimates focusing on the interregional aspects and not all the other sources of uncertainties.” The authors should be consistent in what the objective and the aim of this work is throughout the paper.

Third, the paper should have all the methodologies related to the objective in the paper. For example, if the objective of this paper is indeed on quantifying health impacts, I think the premature mortality calculation methodology should move from the Van Dingenen et al. (2018) to this paper and the crop damage should be taken out from this paper.

Fourth, the writing could be improved, as it is often difficult to follow, as described in minor comments below.

Minor comments:

1. P. 1, l. 29 Not sure what the authors mean by “improve emission inventories knowledge and air quality”
2. P. 2, l. 9 Not sure what the authors mean by “improve globally air quality and possibly human health”

3. P. 2, l. 13-20 I am unsure what the authors mean in the two sentences.
4. P. 2, l. 35 414.000 → 414,000 or 414 thousand
5. P. 4, l. 28 “can be also applied also” → delete the second “also”
6. P. 5 l. 19 “now day much more” → “now much more?”
7. P. 6 l. 10 Not sure what the authors mean by “24OECD90 countries”
8. P. 14 l. 25 How did the authors come up with a threshold value of 5.8 $\mu\text{g}/\text{m}^3$?