The authors coupled the HTAPv2.2 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. They focused on $PM_{2.5}$ due to its negative impacts on human health. The objective of this paper is to evaluate the emissions uncertainties at sector and regional levels, and their propagation in modeled $PM_{2.5}$ concentrations and associated impacts on health. Although the authors state that they have two objectives, I do not quite understand the difference between the two. I find that what the paper is trying to do is important but there are some major problems that need to be addressed before this can be published in ACP.

First, if the objective is to understand the health impacts of $PM_{2.5}$, I believe that the authors need to make sure that their model simulations match with the observations. I do not find the existing comparison in the paper (p. 7, l. 21-28) very convincing. The authors could have at least compared with the recent WHO database of annual $PM_{2.5}$ concentrations at various cities (http://www.who.int/phe/health_topics/outdoorair/databases/cities/en/). For the US, there is much better database that could be used (https://www.epa.gov/outdoor-air-quality-data/interactive-map-air-quality-monitors). The authors seem to allude that it is ok to not include the natural emissions but I disagree and think that the natural emissions need to be included in the model.

Second, I find that the emissions uncertainty estimate seems a little simplistic to only assess within the HTAP inventory, considering the existing differences among various inventories. Also, if the RCP emissions for the year 2000 are used as a baseline, to me it makes more sense to use RCP 2010 in their analysis, rather than switching to HTAP v2.2. Or if the HTAP is to be used, the uncertainty analysis should include the differences in estimates between RCP 2010 and HTAP v2.2. Also, it might be a good idea to compare with some other estimates in existing studies that have estimated emissions uncertainties for certain countries.

Third, I also find it problematic that important details and assumptions of TM5-FASST methodology are described in the paper that is still under preparation. I am assuming that the ΔPMref and ΔEref in Eq. 1 refer to the difference between the TM5-FASST simulation results for $PM_{2.5}$ (and also PM_{10} as well?) using the RCP baseline and the perturbation (-20%) and the emissions themselves, respectively. However, I find it troublesome that these stay constant when the emissions change for all regions and sectors. We know that $PM_{2.5}$ formation is a non-linear process and I do not believe it would work in a linear form for every region for every sector. If it does, maybe that is because simulation uses too coarse of a resolution and the result does not seem realistic. Also, it seems problematic that no explicit treatment of anthropogenic SOA is considered.

Is it correct that TM5-FASST simulations were run for each sector separately and also for all the sectors combined? That is how it looks like from Figure 4. If so, can the authors confirm that the sum of concentrations from each of the sectors run separately are similar to the values when the simulation was done including all the sector emissions together? It would be a nice test to check the linearity in the model. If the simulations were done in this way, then what was the reason equation 1 had to be used? The authors could have easily calculated the impact of each sector using these simulations instead?

I have a hard time understanding the sentence on p. 9 l. 5-8. How do the authors determine the relative contribution to total emission inventory uncertainty? Are the authors using the uncertainty

for a specific sector over the total uncertainty for a specific pollutant as the "average sector relative contribution to total emission inventory"? If so, this does not necessarily take the magnitude of emissions into account and so maybe just looking at this value and deciding which sector to focus on might be a little too simplistic?

Are the upper and the lower boundaries of $PM_{2.5}$ concentrations (Table 2 and Figure 5) calculated based on the linear relationship between emissions per region? In other words, are they simply calculated from emissions, rather than running the simulation again in a chemical transport model?

Minor comments:

1. I would like to see a figure that shows the 10 aggregated receptor regions, as it is unclear, for example, what China+ region includes. Does it just include Mongolia? Or also Korea and Japan?
2. Why are some European countries lumped together in Figure 2 (Austria and Slovenia, for example), whereas others are not?
3. Why are there more countries in Figure 3 than in Figure 1?

p. 2. l. 30-34: The sentence is too long and difficult to understand. Please rephrase the sentence.
p. 2 l. 36-37: The authors state that a second objective of the analysis is to "inform local, regional hemispheric air quality policy makers on the potential impacts of less known emission sectors or regions" but they are focusing on the "6 major anthropogenic emission sectors (l. 6-7, p. 3)." What do they mean by "less known emission sectors" then?
p. 3. l. 19-20. This sentence is not finished.
p. 3. L. 22. Why was such a coarse resolution used, when HTAPv2.2 is much finer?
p. 3 l. 30 relativey → relatively
p. 7 l. 37-39 Perhaps a reference to Bauer et al. (2016) would be appropriate here.
p. 11 l. 36. It is unclear to me where this value (7% for the global non accidental mortalities) is coming from. Can you clarify or cite the source?
p. 12 l. 10 such the Gulf → such as the Gulf
p. 19 Table 2. How do you quantify the uncertainty for a certain pollutant for a region?

Reference:

Bauer, S.E., K. Tsigaridis, and R.L. Miller, 2016: Significant atmospheric aerosol pollution caused by world food cultivation. *Geophys. Res. Lett.*, 43, no. 10, 5394-5400, doi:10.1002/2016GL068354.