**Review of "Exploring the impacts of anthropogenic emission sectors on $PM_{2.5}$ and human health in South and East Asia" by Reddington et al.**

The authors estimated the sectoral contributions to $PM_{2.5}$ concentration and its health impacts in Asia, and they also have made a comprehensive comparison of earlies sector-specific emission studies. Air quality issue in Asian countries is of great concern among scientific communities. The topic is well within the scope of this journal. The methodology in this study is solid, and the conclusions are well defended and discussed. I think the paper is publishable after the following comments/suggestions are addressed.

General comments:
-Agricultural contribution. The authors show a very small contribution (~0.1%) from agriculture sector, which is too low compared with previous studies (Fig.7). In addition, earlies studies (e.g., Zhang et al., 2015; Liu et al., 2019) reported the importance of controlling $NH_3$ emissions, about 90% of which source from agricultural activities, to decrease $PM_{2.5}$ concentration in China. I suggest the authors to re-examine this or at least more discussion on such discrepancy.

-Biomass burning contribution. (1) It's reasonable to have a low estimated contribution from biomass burning emissions, because FINN satellite products tend to underestimate agricultural fire emissions in China (Shen et al., 2019). However, the simulated spatial contribution from biomass burning (Fig. S2c) is not very consistent with other studies (e.g., Li et al., 2016). It will be better for readers to understand the numbers in this work if there is more discussion on this. (2) The authors also should note there is strong interannual variations for biomass burning emissions. (3) when saying the contribution from biomass burning, the authors used words like "excluding fire emissions" (P12L5, P12L20). In fact, wildfire emissions are not so controllable as those from anthropogenic fires. Should make it more clear in the text.

Specific comments:
-P5L15. Anthropogenic emissions from HATP are for year 2010. Are all the species compiled from regional emission inventories for year 2010. Please make it clear in this section.
-Correlation between simulated and observed $PM_{2.5}$ in India is only 0.37, though regional mean values are not biased. More discussion on the model capability to simulate $PM_{2.5}$ in India is needed.
-In Fig.4 and Fig.5, I suggest to mask the sectoral contribution over oceans where the value reads kind of weird.
-In Fig.6, Fig.6b and 6c are like almost the same. It is reasonable to show only one (e.g., Fig.6b).
-P38L5: in Fig.7 caption, please add a "(*)" after the text "in the legend with an asterisk". A symbol is more catching than a word in text.

Reference:

1. Li et al. (2016). Source sector and region contributions to concentration and direct radiative forcing of black carbon in China. *Atmos. Environ.* 124, 351-366. 2. Liu et al. (2019). Ammonia emission control in China would mitigate haze pollution and nitrogen deposition, but worsen acid rain. *PNAS*, 201814880. 3.Shen et al. (2019). 2005-2016 trends of formaldehyde columns over China observed by satellites: increasing anthropogenic emissions of volatile organic compounds and decreasing agricultural fire emissions. Geophys. Res. Lett. 4.Zhang et al. (2015). Source attribution of particulate matter pollution over North China with the adjoint method. *Environ. Res. Lett.* 10(8):084011.