

Interactive comment on “Long-range Transport Impacts on Surface Aerosol Concentrations and the Contributions to Haze Events in China: an HTAP2 Multi-Model Study” by Xinyi Dong et al.

Anonymous Referee #1

Received and published: 24 May 2018

The manuscript submitted by Dong et al. reports a basic statistical analysis of 6 simulations from HTAP2 global modelling exercise, aimed at assessing the simulated impact of long-range transport of pollutants from Europe and Russia on China's haze events. The scope of the work is well defined, I think there is some gap that may be filled in terms of link with the existing literature, and there is generally no attempt by the authors in explaining the reasons for inter-model differences. The manuscript is basically a description, sometimes lengthy, of the materials presented in the figures and the tables. Considering the relevance of the topic, I think the manuscript could be published on ACP, after considering some suggestions given below, and after careful English editing.

Specific comments:

1. In the introduction the authors very briefly review the literature regarding existing studies on haze in China. It is mentioned that long-range transport contribution to haze episodes is poorly documented (indeed they do not insert any reference). However, the literature on long-range transport to China is not null, and part of the phenomenology and underlying mechanisms might be in common with period of haze episodes. From a very quick literature search I identified, as potential references:

- Lee et al., Heavy metals and Pb isotopic composition of aerosols in urban and suburban areas of Hong Kong and Guangzhou, South China—Evidence of the long-range transport of air contaminants, Atmospheric Environment, Volume 41, Issue 2, January 2007, Pages 432-447

- Kong et al., Receptor modeling of PM2.5, PM10 and TSP in different seasons and long-range transport analysis at a coastal site of Tianjin, China, Science of The Total Environment, Volume 408, Issue 20, 15 September 2010, Pages 4681-4694

- Akimoto, Global Air Quality and Pollution, Science 05 Dec 2003: Vol. 302, Issue 5651, pp. 1716-1719 (and references therein)

I suggest to deepen the review of the literature on long-range transport from Europe to East Asia and put it into the fourth paragraph of the introduction. The same material might be subsequently used in the interpretation of some of the results illustrate afterwards (e.g. in section 3.1 and 3.2.

2. page 4, lines 1-5: I think these very general statements, without any specific reference, on physical processes should be avoided in the manuscript. Please add proper reference and try to be more specific on the region and the situation you are referring to.

3. section 2.2: all the data versions and source of data are missing. Please add the exact product names of the data used, the web source used, and the version of the

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algorithms. This is necessary for the reproducibility of the work.

4. Figures 2 and 3 and related comments: there are some apparent inconsistency between the results presented in these figures. For example, PM_{2.5} is overestimated by GEOSCHEMADJOINT and underestimated by CHASER, but then AOD at AERONET sites has the opposite bias for these models. Why is that? Perhaps it could be useful to include a comparison only for some specific station for which all the datasets are available, or at least within the same model grid. From Figure 1 it seems to be possible for some stations.

5. Figure 4 and related comments: the modelled AOD over China and elsewhere in the domain differ among models by more than a factor of two. As for previous results on point measurements, there is no attempt to explain the differences. For example, considering the same anthropogenic emissions, the difference over China CHASER and SPRINTARS is quite remarkable.

6. Figures 5-6 and related comments. The figures are interesting because they nicely illustrate the model diversity. For example, the seasonal cycle of contributions from some models is opposite to that of others (e.g. CAM-Chem peaks in summer, CHASER in winter, and GEOS5 in spring). It would be useful to have some inspection of these difference. I suspect that differences in the meteorological fields used in these models are responsible for the large variability.

7. Figures 9-10: some panels look patchy, for example EMEP, SPRINTARS and all in Figure 10. Why is that?

8. I recommend English editing of the manuscript. The use of language is imaginative and makes understanding difficult. A few random examples:

- p. 3, l. 40-41: "These datasets are essential to estimate surface PM response compare the aerosol transport in different atmosphere layers". What is "response compare"? "atmosphere" → "atmospheric"

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- p. 5. l. 6-7: "the models all tend to underestimate the high peaks in spring (Mar.-Apr.) and low bottoms in summer". Not clear what "low bottoms" means.

- note 2 on caption of Figure 2: "PM2.5 observations in EUR and EAS region have no standard because there are no sites with valid measurements fall into the same model ensemble mean grid". Very difficult to understand: why a standard deviation cannot be calculated even if stations are not in the same model cell?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-91>, 2018.

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