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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global and regional radiative forcing from 20 BC, OC and SO_4 – an HTAP2 multi-model study" *by* Camilla Weum Stjern et al.

Anonymous Referee #2

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The manuscript by Stjern et al. examines the local and remote influences of aerosol and aerosol precursor emissions on regional atmospheric aerosol abundances and radiative forcing. This is achieved using results from 20% regional emission reduction experiments performed as part of the Hemispheric Transport of Air Pollution Phase 2 (HTAP) exercise in conjunction with pre-calculated aerosol forcing efficiencies. The authors document the various responses, and towards the end they additionally explore the influence of the vertical distribution of aerosols on the results. The manuscript is well written and certainly within the scope of Atmospheric Chemistry and Physics. Even though there are no particularly novel findings in it, the fact that it documents and thoroughly discusses the results of these important new multi-model experiments

makes it worth publishing, following some (mostly minor) corrections that I suggest below.

SPECIFIC COMMENTS:

Abstract: I suggest that the authors clearly state that in most cases the local influence is the dominant, though with important contributions from remote regions. Then to proceed with outlining specific features, as the already do.

Page 2, Line 69: What is meant by "efficiency"? Presumably "radiative forcing efficiency". Please clarify.

Page 3, Lines 105-108: Presumably the three numbers refer to the three different species. But which region do they refer to?

Page 4, Line 131: Does the climate correspond to the specific year 2010 in the models, or to climatic conditions representative of years "around" 2010. Please specify, and if the former, please discuss how the choice of a single year may affect the conclusions.

Page 4, Line 155: I suggest changing "to abundance" to "to column abundance", as "abundance" is a general term that can refer to pretty much anything (including the MMRs).

Sect. 2.2: It should be clearer to a reader not familiar with the Samset and Myhre (2011) manuscript how the OsloCTM2 was utilized for calculating those AFEs. Initially it is stated that a radiative transfer code is used for calculating the AFEs, with the OsloCTM2 providing the background aerosols. However, later it is mentioned as "the host model", and that "the absolute RF will be influenced by the mean efficiency of the host model". Please clarify.

Page 4, Line 162: Suggest changing to "emission and subsequent concentration reductions".

Page 4, Line 165: What does "a series of simulations" mean here? Please explain.

Page 4, Line 168: Please provide reference justifying this adjustment.

Page 4, Line 169: What type of scaling was used? Reference?

Page 5, Line 175: I would say that it would be useful to give a brief summary of the impact of using a single model's kernel in this manuscript too, given how central AFEs are for the results presented here. No need to be long – a few sentences would suffice.

Page 5, Line 177: I presume by "resulting" it is meant "modelled"? If so, please change, clearly mentioning that this is from the new HTAP2 simulations.

Page 5, Line 178: Please remind the reader that "time" here corresponds to the month of the year.

Page 6, Lines 214-215: Suggest rephrasing the part of the sentence after the second comma with "which gives an estimate of inter-continental transport in two dimensions (ignoring the vertical)".

Page 8, Lines 301-312: This is a peculiar feature and needs some further explanation. It is a bit too hand-wavy to say that nudging may be responsible. If so, why would it mainly appear in SO4 and OA, but not BC? Maybe it has to do with effects of aerosol emission reductions on oxidants in the models? SO4 and OA would be affected by this, but not BC.

Page 8, Lines 315-319: Is this in agreement with what other studies that examined long-range transport of pollution to the Arctic (e.g. Shindell et al., 2008) have found? Worth mentioning.

Figure 6: I suggest reminding the reader in the caption that these are inferred from one model, and which model this is.

Page 9, Line 346: I suggest clarifying that panel (a) is for BC. This is more important than mentioning the panel.

Page 9, Lines 354-356: Worth mentioning that the vertical increase in the Middle East

is not as steep, presumably due to the lower occurrence of clouds in this area.

Page 10, Lines 387-388: Could it also be the lower insolation in this region?

Page 10, Line 402: Worth citing the recent paper by Kasoar et al. (2016) here, as it also discusses thoroughly the causes of diversity in three different models when it comes to simulating climate impacts of identical regional SO2 emission perturbations.

Table 4: I may be missing something, but shouldn't the values in Table 4 be consistent with the values in Figure 10? I cannot see this fully being true: For example, for the first bar in Fig. 10 - representing North America - the domestic influence seems to be responsible for much more than half of the SO4 forcing, but then in Table 4 it appears as if it is just above 60%. Please check consistency (for the whole table) and/or explain.

Table 4: I suggest mentioning what the range indicated next to the means represents.

Page 13, Line 520: Please add "surface" before "albedo".

Page 13, Lines 526-530: As mentioned in the abstract as well, I suggest that you clearly state at the beginning of this paragraph that in most cases the local influence is the dominant, though with important contributions from remote regions. And then proceed with outlining the cases where remote is stronger than local (as is done already).

Page 13, Lines 531-533: In the first sentence of this paragraph, it is mentioned that the effect of "vertically resolved concentrations" is examined, while in the next sentence it is stated that "Using vertically resolved AFE distributions strengthens. . .". Which of the two is examined, the influence of smoothed concentrations or of smoothed AFEs? Earlier it is mentioned that the effect of both is examined. Please clarify.

REFERENCES:

Kasoar, M., Voulgarakis, A., Lamarque, J.-F., Shindell, D. T., Bellouin, N., Collins, W. J., Faluvegi, G., and Tsigaridis, K. (2016), Regional and global temperature response to anthropogenic SO2 emissions from China in three climate models, Atmos. Chem.

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Shindell, D. T., Chin, M., Dentener, F., Doherty, R. M., Faluvegi, G., Fiore, A. M., Hess, P., Koch, D. M., MacKenzie, I. A., Sanderson, M. G., Schultz, M. G., Schulz, M., Stevenson, D. S., Teich, H., Textor, C., Wild, O., Bergmann, D. J., Bey, I., Bian, H., Cuvelier, C., Duncan, B. N., Folberth, G., Horowitz, L. W., Jonson, J., Kaminski, J. W., Marmer, E., Park, R., Pringle, K. J., Schroeder, S., Szopa, S., Takemura, T., Zeng, G., Keating, T. J., and Zuber, A. (2008), A multi-model assessment of pollution transport to the Arctic, Atmos. Chem. Phys., 8, 5353-5372, doi:10.5194/acp-8-5353-2008.

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