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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.

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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.

Response: We agree that this should be stressed, and have now added this to the abstract.

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

Response: That is correct. This is now clarified.

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

Response: This was indeed unclear. The numbers are, as given in Yu et al, averages over all four regional emission-reduction experiments, and we have tried to express this more clearly now.

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.

Response: The climate corresponds to the specific year 2010, and the reviewer is of course right that the results of this paper will be affected by that. We have added a couple of sentences discussing this.

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).

Response: We agree that the specification is necessary, and this is now fixed.

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.

Response: This section has been revised and clarified. It now reads: "In order to estimate the radiative forcing resulting from the emission and subsequent concentration reductions simulated by the HTAP2 experiments, we utilize precalculated 4D distributions of aerosol forcing efficiency (AFE), which is defined as the RF per gram of a given aerosol species. For the three aerosol species, AFE was calculated for each grid

cell and month by inserting a known amount of aerosol within a known background of aerosols and clouds, for each model layer individually, and calculating the resulting radiative effect using an 8-stream radiative transfer model with four short wave spectral bands (Stamnes et al., 1988). I.e. the model was used to calculate the response to a change in aerosol concentration at a given altitude, and run for a whole year to capture seasonal variability. The simulations for different model layers were then combined into a set of radiative kernels, one for each aerosol species. For the radiative transfer calculations aerosol optical properties were derived from Mie theory. The absorption of aged BC was enhanced by 50% to take into account external mixing, as suggested by Bond and Bergstrom (2006), and for all models we assume the same mixing ratio between aged and non-aged BC as in OsloCTM2. Hygroscopic growth of SO4 was included, scaling with relative humidity according to Fitzgerald (1975). See Myhre et al. (2004) for a discussion on the impacts of this choice. For OA, purely scattering aerosols are assumed. Background aerosols were taken from simulations using OsloCTM2. See Samset and Myhre (2011) for details, but note that all numbers have been updated since that work, taking into account recent model improvements (Samset and Myhre, 2015). The resulting AFE profiles, averaged over the individual regions from Fig. 1 (a), is presented in Sect. 3.3. For a full discussion on the impact on radiative forcing from using a single model kernel, see Samset et al. (2013). Briefly, multi-model average forcing becomes representative of that of the most model, including cloud fields and optical properties, while the variability around this value is indicative of the impact of differences in 3D aerosol burdens. The resulting reduction in multi-model relative standard deviation depends on the regional and vertical differences in AFE, but is generally less than 20%."

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

Response: This is now changed.

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

Response: See the revised section above.

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

Response: A reference to Bond and Bergstrom 2006 ("Light Absorption by Carbonaceous Particles: An Investigative Review", Aerosol Science and Technology) is included

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

Response: We have now specified this in the text, and provided references; "Hygroscopic growth of SO4 was included, scaling with relative humidity according to Fitzgerald (1975). See Myhre et al. (2004) for a discussion on the impacts of this choice."

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.

Response: See the revised section above

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.

Response: The sentence now reads "The direct RF from a given aerosol species due to a 20 % emission reduction was then estimated by multiplying the modelled aerosol burden change profile ?BD (rom a given HTAP2 model and experiment) with the OsloCTM2 AFE distribution for that species and point in space and time (month of the year).", which is hopefully more clear.

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

Response: This information is now added.

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)”.

Response: We agree that this improves the sentence and have followed the reviewer’s suggestion.

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.

Response: We appricate the useful suggestion and have included the following sentences emphasizing oxidant changes as a potential cause of the concentrations changes: “Regional increases in aerosol concentrations imposed by emission reductions can be observed for SPRINTARS and CAMchem, and to a smaller extent also for the CHASER models, GEOS5 and C-IFS (not shown, but visible in the globally averaged RBUreduced and MDEreduced plots for OA in Fig. 4). This occurs mainly for OA and SO4. Conceivably, aerosol emission reductions may in these models be influencing the level of oxidants, which would have feedbacks on the concentrations of OA and SO4. A model study by Shindell et al. (2009) demonstrates the importance of aerosol-gas interactions to the climate impact of mitigations. They point out that the effect on oxidant changes on SO4 concentrations are stronger in oxidant-limited regions with high SO2 emissions, and that greater parts of the industrialized Northern Hemisphere is, in fact, oxidant limited (Berglen et al., 2004)”

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.

Response: Thank you for this comment; consistency with previous studies is indeed relevant to mention. We do see some similarities between the Shindell et al. paper

and ours: high-altitude changes in pollution levels in the Arctic tended to originate from East and South Asia, while low-level changes were dominated by changes in Europe. In addition we see a strong influence on the Artic from Russia, which is also seen in other studies (Sand et al., 2013b; Stohl, 2006). We have included a few sentences on this at the given location in the text.

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

Response: This information is now added to the caption.

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

Response: We agree, and have now rephrased the sentence to clarify this.

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.

Response: Absolutely, we have now included a sentence on this.

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

Response: Lower insolation could at least be a contributing cause for the lower AFE values in Russia, and we now include this in our suggested explanation.

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.

Response: The paper, which is also part of the ACP special issue on HTAP, should indeed be cited, and is now included among the other references.

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.

Response: This is well spotted, and we understand that the differences look incoherent. The reason that the numbers are not equal is that for the RERER calculations, numbers (e.g., domestic contribution to the RF) are calculated relative to the experiment where global emissions were reduced by 20%. Conversely, in Figure 10, the corresponding numbers are calculated relative to the sum of the given region's forcing caused by all the six major experiments. As the summed RF following emission reductions in our six source regions is not quite as large as a 20% emission reduction all over the world, the RERER numbers in Table 4 are relative to larger numbers and will therefore be smaller than the corresponding numbers of Figure 10. We have tried to express this in the text.

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

Response: That information should definitely be there – this is now fixed.

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

Response: The word is now added.

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).

Response: We have added an extra couple of sentences here to stress this.

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.

Response: This was indeed unclear; we have now rephrased this section to clarify.

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