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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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Was direct aerosol forcing calculated internally by any of the contributing aerosol models? If so, it would be very helpful to compare these forcing estimates with those obtained by running the aerosol fields through the authors' radiative kernels. This could offer an indication of how much additional variability in forcing might be expected from different model cloud fields, assumed aerosol optical properties, and radiative transfer codes.

Response: This was indeed considered, as we agree that much could be learned from such an analysis. No participating model group initially performed the RF calculations, but we were in contact with one group on adding one sensitivity test. In the end,

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Discussion paper

however, they were not able to deliver the results and we decided to proceed without the analysis. For a full analysis where both native mode RF and kernel estimates was available, however, see Samset et al. 2013, ACP, "Black carbon vertical profiles strongly affect its radiative forcing uncertainty", Figure 1. There, it was found that for BC, between 20 and 50% of the variability can be attributed to vertical profiles alone, with the rest being due to a combination of optical properties, horizontal transport and differences in cloud fields. Also note that Stier et al. 2013 (ACP, "Host model uncertainties in aerosol radiative forcing estimates: results from the AeroCom Prescribed intercomparison study ") investigated model uncertainty in direct RF for twelve AeroCom models and found substantial diversity in both clear- and all-sky RF even when aerosol radiative properties were prescribed. For HTAP2, a followup study with 2-3 models may be performed later.

Abstract: It would be helpful to include some of the overarching quantitative results in the abstract, namely the model-mean changes (and perhaps inter-model standard deviation) in global radiative forcing resulting from the emissions perturbations. I view these numbers as headline results from the study that should be reported in the abstract.

Response: We absolutely agree, and we have tried several approaches to this in earlier manuscript versions. However, the "main results" comprise forcings from emission changes in six different regions, for three different species. Obviously, a listing of eighteen numbers in the abstract is less than ideal. We therefore chose to list the ranges of RF resulting from emission reduction in the six regions, as the numbers vary so much between the regions.

Introduction (and last sentence of abstract): I suggest mentioning that although BC radiative efficiency increases with BC altitude, the associated surface temperature change does not, and can even become opposite of the sign of TOA forcing when the BC is located at sufficiently high altitude.

Response: This is an important point, and we now comment on this in both abstract and introduction. In the abstract, we added the text: “In the present study, it has only been possible to estimate the effects of emission reductions on instantaneous top-of-atmosphere RF from the direct effect. As the climate response efficiency to aerosols, in particular that of BC, also depends on altitude, we do not extend our analysis to estimates of temperature or precipitation changes.” In the Introduction, we added the text: “Previous studies have shown that the relationship between instantaneous RF, which is what we estimate here, and the surface temperature change following a change in BC also depends on the altitude of the BC. Although not found in all studies (Ming et al., 2010), there is a tendency that BC inserted near the surface causes warming, whereas BC near the tropopause and in the stratosphere causes cooling (Ban-Weiss et al., 2012; Samset and Myhre, 2015; Sand et al., 2013a; Shindell and Faluvegi, 2009). This is mainly related to the semi-direct effect of BC, which causes a negative RF through suppression of cloud formation by enhancing atmospheric stability, and which is not accounted for when calculating the instantaneous forcing via radiative kernels. It is beyond the scope of study to calculate climate change in terms of surface temperature change, and we stress that a positive/negative estimate of direct RF here should not be translated directly into warming or cooling.”

line 75: "on" -> "of"

Response: The word is changed.

line 105-109: How do the HTAP2 results compare with these HTAP1 results?

Response: We do provide some comparison (Section 3.3, third paragraph) of emission-weighted radiation changes between HTAP1 (Yu et al.) and HTAP2. HTAP1 and HTAP2 had different sets of contributing models, and we do comment on this and other causes of HTAP1-HTAP2 differences in this and the following sections.

Section 2.1, line 132: Please list the global annual emissions of each species, as represented in the inventories applied.

Response: The emission numbers have been included in this sentence.

Section 2.1, paragraph 2: Please mention here that all models use prescribed meteorology (rather, e.g., than prescribed SSTs with online meteorology), assuming this is indeed true.

Response: That is correct, and we now specify this.

lines 155-158: How much error does this interpolation technique introduce to the estimates of column-integrated aerosol abundance (burden), as opposed to using each host model's pressure fields self-consistently with their aerosol fields? It is probably small, but worth mentioning. (And why were these calculations done with OsloCTM pressure/mass fields instead of the native model's fields?)

Response: The reason that the calculations are performed with fields from one model, is primarily that the input data was not readily available from the other models. Further, as we interpolate to a common vertical dimension for comparability anyway, this method entails the fewest interpolations. For previous analyses with AeroCom Phase II data, the interpolation has shown to change column burdens by less than 1%.

Section 2.2, paragraph 1: What spectral resolution (or how many spectral bands) was applied in the radiative transfer calculations?

Response: Four short wave bands. The text has been updated.

line 168: Did all models provide separate mixing ratios for "aged" and "non-aged" BC? If not, how did you partition the BC fields into these two components for the radiative forcing calculations?

Response: We assume the same mixing ratio as in OsloCTM2 for all models. This information is now added at this location.

line 180-181: I assume that the forcings presented here are instantaneous forcings, rather than adjusted or effective forcings, but please clarify this.

Response: Yes, the forcings are instantaneous, and this is now stressed.

line 194: Are the vertically-averaged AFEs weighted by aerosol mass? (presumably so).

Response: Yes. They stem from separate calculations where a realistic (i.e. stemming from AeroCom Phase II emission) vertical profile has been used. Also, on practice, this would affect the overall scaling, but not the analysis of change in inter-model variability (for comparison to Yu et al. 2011, for instance) so long as the same 2D field is used systematically.

Section 3 / Figure 1: I think it would also be quite useful to show/describe the intermodel variability (e.g., standard deviation or normalized standard deviation) in aerosol burden. This would provide a nice depiction to readers of where the models tend to differ from each other the most. Deviation plots could either be included in Figure 2 or added as a separate figure.

Response: This is indeed relevant information. We have created relative standard deviation plots for the three species and included them in Figure 2 as suggested. In general, the models disagree the most over the tropics and over the poles, and we comment upon this in the text.

Lines 301-312: Could non-linear chemical processes provide an alternative explanation for this odd behavior of increasing aerosol concentrations in response to emissions reductions?

Response: Yes it could. We have previously been in close contact with the modelling groups of the models showing this unexpected increase, and have had no suggestions as to the cause, other than the nudging. This is, however, only a suggested cause, and the alternative explanation suggested by the reviewer is just as valid. We have therefore extended this section, including a few sentences on oxidation feedbacks.

lines 394-402: Are the intermodel differences in radiative forcing larger or smaller than the differences found by Yu et al (2013) for HTAP1 simulations? Presumably they are smaller because of the use of identical emissions data in HTAP2, but it would be helpful to provide a semi-quantitative comparison of the inter-model variability between these two studies.

Response: We agree that such a comparison would be helpful. Yu et al. (2013) do provide emission-weighted standard deviations of emission-weighed forcing, which we have translated to relative standard deviations and compared to our numbers from Table 3. The different meteorological years used for the two analyses, as well as the different set of models (only CTMs In Yu et al and a mix of CTMs and GCMs here) and the different region definitions, precludes a proper quantitative comparison, but we do give the numbers as well as a short discussion at this place in the text.

line 499: "...13% of global deaths" - Is this 13% of the deaths caused by inhalation of fine particulate matter? Please clarify.

Response: Yes it is – this is now clarified in the text.

Figure 1: Do the median fields shown here represent fields from a single (median) model, or is the median computed at each gridcell from all models? Please clarify.

Response: Median fields are calculated in each grid point – this is now clarified in the figure caption.

Figure 2: Are the SO2 emissions reported in Tg of S or Tg of SO2? Please clarify.

Response: SO2 emissions are reported in Tg SO2, as now stated in the figure caption.

Figure 8: It seems the units here should have a vertical component, e.g., mW/m2/Pa or mW/m2/m or mW/m2/layer. Is this so? Otherwise, how does one obtain the typical column radiative forcing (W/m2) from these vertical profiles? Please clarify.

Response: Yes, the unit is supposed to read mW/m2/layer – this is now fixed in the figure.

Table 2: It would be helpful to also include the multi-model standard deviations for convective mass flux, precipitation, and cloud, if at all possible.

Response: That would indeed have been interesting to see, but this information is regretfully not available to us.

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