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Atmospheric
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Discussions

Interactive comment on "Influence of future air pollution mitigation strategies on total aerosol radiative forcing" *by* S. Kloster et al.

Anonymous Referee #2

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Review of "Influence of future air pollution mitigation strategies on total aerosol radiative forcing" by S. Kloster et al. (acpd-2008-0023)

This manuscript describes the use of a coupled aerosol-climate model to estimate the direct and indirect radiative forcing that would occur under two different emission scenarios for the future (year 2030). The aerosol radiative forcing from pre-industrial to present times is also calculated. A large number of sensitivity simulations are conducted in order to explain the factors contributing to the future changes in aerosol burdens and forcings.

The paper is quite long, but contains significant and interesting results. Few of the results from the sensitivity simulations are included in the abstract, so one possible

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way of shortening the article would be to cut some of these simulations or move them to supplementary material (as has already been done with some of the figures and tables). The main results contained in the paper warrant publication, but there are currently what seem like many inaccuracies and inconsistencies in the paper that need to be sorted out before this paper can be published. I detail my major concerns and minor comments below.

Major Comments ————

1) Model description

The model description is broken up somewhat awkwardly between Sections 2 and 3. There are many aspects of the model description that would seem to fit more naturally in Section 2 that are withheld until Section 3, or in some cases not given at all. For instance, the use of offline oxidants should be mentioned in Section 2.2 where HAM is described. You do not adequately describe how aerosols are converted from hydrophilic to hydrophobic (relevant for the discussion of "microphysical aging time"). There is some description later (Section 5.2), but none in the model description sections. Which variables are being nudged in your model? Any of the cloud fields, or only the "dynamical" fields? What about water vapor?

2) Emissions

Aerosol emissions are discussed in Section 3.1 and Table 1. But, it is hard to understand which of the emissions shown in Table 1 are included in any particular simulation. Table 2 helps with this, but doesn't clear up all of the confusion. Some of the values given in Table 1 don't seem to be used in any of the simulations (e.g., "anthropogenic AEROCOM 2000"). Maybe you could add another row at the bottom of Table 1 that lists the total emissions for the 4 base simulations (1750, 2000, CLE 2030, and MFR 2030). You could also bold the individual entries in the Table that are included in those totals.

An additional problem with the emissions is that some of the values in Table 1 don't agree with those given by Dentener et al. (2006a). This either needs to be corrected or explained (clearly) in the text.

3) Comparison with previous studies

There are few comparisons of the results of this study with those of previous studies. For instance, the year 2000 aerosol budgets (Section 4.2) can be compared with the range of results presented in the Textor et al. 2007 AEROCOM paper. You state that the -2.0 W/m2 aerosol RF from preindustrial to present is on the high end of models reported in Denman et al. 2007. What are the implications of the large negative preindustrial to present RF for the total (gas+aerosol) forcing over this time period? There have been other projections of future aerosol radiative forcing (at least for direct forcing, possibly also for indirect) with which you could compare.

4) "SO2 emissions" versus "SO2 source"

In Section 4.1, you repeatedly refer to "SO2 emissions" in giving percent changes between simulations. Do you really mean SO2 emissions (as in Table 1) here, or are you referring to "SO4 source" (as in Table 3). Comparing the numbers given in the text, it appears that you are referring to the SO4 source, although this is extremely misleading. For instance, you state that the SO2 emissions increase by 6% in the CLE:2030 experiment. As far as I can tell (see point 2 above), the total SO2 emissions in 2000 are 74.14 Tg/yr, and in CLE:2030 are 77.2. This is an increase of 4%. From Table 3, though, the SO4 source increases from 70.49 to 74.85, an increase of 6%. Please clarify throughout this section (and sporadically in other sections).

5) Reference simulations (PI, present, future)

The choice of reference years for showing differences and, especially, percent differences throughout the paper is unintuitive and leads to some misleading statements. For instance, in Section 4.1 you refer to "the increase of SO2 [emissions] from pre-

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industrial to present day times (+55%)". Aside from the fact that you are referring to SO4 source, not SO2 emissions (see comment 4 above), this is still confusing. From Table 3, the SO4 source increases from 31.46 in PI to 70.49 in 2000. This is a 124% increase, not a 55% increase. What you mean to say here is that PI is 55% lower than present. The usage in the paper is sloppy and misleading, and needs to be cleaned up.

Changing the reference years for the RF calculations (as is done in Tables 4 and 5) further confuses the issue.

Minor comments ————

Abstract

p.5564, l.21 – Typo, "extend" should be "extent".

2.2 The aerosol model HAM

p.5568 – In the description of the sulfur scheme, it should be mentioned that offline oxidant concentrations are used. You should also either mention the source of these fields, or indicate that they are discussed in a later section.

pp.5568-5569 – Mention in this section how hydrophobic aerosols are converted into hydrophilic form. How much coating (of sulfate or other materials) is needed before the aerosols are considered hydrophilic?

2.4 Model evaluation

p.5571, l.4 – Quantify the underestimate of observed BC concentrations.

p.5571, l.4 – Give values for the lifetimes or refer to the appropriate Tables. Do you mean here that the lifetime of BC is almost identical to that of POM, or that the lifetimes are almost identical to those in the "reference simulation"? Clarify.

p.5571, l.9 – Qualify your statement about "good agreement" by referring back to the biases mentioned in the previous paragraph.

3 Simulation setup

p.5571, l.15-19 – Which variables are being nudged? Over which altitudes?

p.5571, l.20-25 – An additional difference between the RF as calculated here and in the standard definition is this lack of stratospheric adjustment (although this has been found to be unimportant for tropospheric aerosols).

3.1 Aerosol emissions

p.5573, l.4 – This is the first mention of a pre-industrial simulation. This should be discussed earlier (e.g., in the Intro) along with the other main simulations.

p.5573, l.3-6 – This sentence is unclear. Indicate that the biomass burning emissions are different in 1750 versus 2000, but that the 2000 values are assumed to apply also in 2030. Mention that the production of SOA is treated via direct emissions of POM (as prescribed in AEROCOM). Also, clarify that 1750 emissions for other source categories are taken from AEROCOM (Dentener et al., 2006a).

3.2 Oxidant concentrations

p.5573 – Clarify that different oxidant fields are used for the pre-industrial, present, CLE:2030 and MFR:2030 experiments. Mention that the details are provided in the next section.

3.3 Description of the single experiments

p.5575, l.6-14: Explain which set of oxidant concentrations are used for these sensitivity experiments.

4.1 Aerosols emissions

p.5575, l.20 – Refer also to Table 3 here.

p.5576, l.6 – Do you mean "SO2 emissions" or "SO4 source" here? See also major

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comment #4 above. Same comment throughout this paragraph and on p.5577, l.14.

p.5576, l.8 – Explain why there is a range given for Africa but not for the other regions. Presumably this refers to the 4 different Africa regions shown in Table A1 (Northern Africa, Western Africa, Eastern Africa, and Southern Africa – note that the colors for these regions in Fig. A3 are almost impossible to distinguish). In the case of Europe, though, you cite the value for "Europe OECD" rather than "Eastern Europe", "Europe Reg. Exp." (which is never defined). Why? Elsewhere in the text (e.g., Section 4.3, p.5581) you seem to use the "Europe Reg. Exp." region instead. Similarly, the value you give for "North America" in the text is listed in the table as "USA". Be consistent in the names you use for regions. As an additional point, why does Table A1 give "SO2 source" while Tables 3-5 give "SO4 source". This is confusing.

p.5576, l.11 – Add "respectively" after "-28% and -13%".

p.5576, l.11-12 – Confusing. The SO4 source increases by 124% from PI to 2000. You mean here that PI is 55% lower than present. (See major comment #5 above.) Instead, you can just say that the MFR:2030 SO4 source is reduced nearly to PI levels (33% higher).

p.5576,l.13-14 – Again, confusing. See previous comment. Also, these POM values don't match up with Table 3. Correct values here or in Table.

p.5576,l.23 – But, Table 3 shows a -2% decrease in the SO4 source in MFR:2030:DT vs. a 6% increase in CLE:2030. Rewrite this sentence.

4.2 Aerosol burden and aerosol optical depth

p.5577, l.14 – Do you mean "emission" or "SO4 production" here?

p.5578, l.8 – Again, confusing. The AOD actually increases by 34% from PI to 2000. The 25% you refer to is the *decrease* of AOD from 2000 to PI, i.e., (PI-2000)/2000 * 100% = -25%.

4.3 Aerosol forcing

p.5579, l.17-19 – The RF also differs from the standard definition by not including stratospheric adjustment, but as mentioned above this is probably not important for aerosols. Also, mention that you calculate RF at TOA rather than at the tropopause.

p.5579, l.21 – Change "reflects" to "indicates changes in" to avoid any confusion.

p.5580, l.16 – The TOA RF perturbation in MFR:2030 is given as +1.13 W/m2 (not 1.03) in Tables 3, 4, and A1, and in Figure 2a.

p.5580, l.27-29 – Explain which experiments in Tables 3-5 were used for this calculation of the effects of reducing BC and POM emissions. Sulfate:2000 versus 2000? Versus 2000:chem:2030:MFR? Unclear.

p.5581, l.4-5 – This sentence has the sign flipped. Applying MFR emissions over Europe causes a small *negative* RF. The RF is reduced from +0.02 in CLE:2030 to 0.00 in MFR:2030:EUROPE.

p.5581, l.6-8 – Table A1 shows that the RF over "Europe Reg. Exp." is +2.93 in MFR:2030 and +1.932 in MFR:2030:EUROPE. That means that RF in MFR:2030 is 52% higher (not 34%) than in MFR:2030:EUROPE. Also, you need to define "Europe Reg. Exp." somewhere.

p.5581, l.15 – If you are referring to the "Asia Reg. Exp." region here, then the increase in RF in MFR:2030 versus MFR:2030:ASIA is 23% (not 20%) according to Table A1.

5.1 Influence of oxidant concentrations

p.5582, l.25-27 – Clarify that you mean here small impacts versus the standard MFR:2030 and CLE:2030 runs (not small impacts versus 2000).

5.2 Changes in aerosol composition

p.5584, l.17-20 – Expand on the treatment of microphysical aging in the model earlier

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in the paper, i.e., in the model description section(s).

5.2.1 Additivity of the radiative forcing

p.5586, l.8 – Typo, "condensates" should be "condenses".

6 Discussion and conclusions

p.5587, l.17 – Indicate that this RF perturbation is vs. 2000.

p.5587, l.26-28 – This sentence is unclear. First, as mentioned in an earlier comment, applying MFR in Europe vs. the CLE:2030 scenario produces a small *decrease* in the RF (-0.02 change, from +0.02 to 0.00). Since you already gave the values for MFR:2030:EUROPE and CLE:2030 separately in the preceding few sentences, I would suggest cutting this sentence entirely. Same point concerning the final sentence of this paragraph. Try not to keep switching between Europe and Asia in this paragraph. First, discuss Europe fully, then Asia.

p.5588, l.13-15 – It is unclear what you mean by "with a cooling of the atmosphere due to less absorbing aerosols present in the same magnitude". Which experiments were used to calculate the -0.14 W/m2 value here? (See also comment on p.5580.)

Table 1 – Several of the AEROCOM values don't seem consistent with the values given by Dentener et al. (2006a). For instance, you list anthropogenic emissions for 1750 as 2.0 Tg/yr for POM and 0.8 TgS/yr for SO2. Dentener et al. seem to list these values as 1.56 Tg/yr and 0.06 TgS/yr, respectively. Minor discrepancy for SO2 from natural sources (14.8 vs. 14.6). Explain. Also, as mentioned above (major comment #2), it would help to indicate more clearly which values in Table 1 are used in the simulations.

Table 3 – The SO4 source percent change for MFR:2030:DT should be -2, not +2.

Table 3 – What do you mean by "transfer of the hydrophilic modes to the hydrophobic ones"? Do you mean chemical conversion of hydrophobic to hydrophilic? Explain more clearly in the text how this is calculated.

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Tables 4-5 – These tables repeat a lot of information from Table3. I would prefer to see these tables combined into a single table (using a consistent base year for the forcing perturbations). Right now, it is unclear why certain information is given in one table vs. another. Why are some of the rows in Table 3 left out from Tables 4-5?

Table A1 – You need to define "Europe Reg. Exp", "Asia Reg. Exp." Also, be more clear about which regions you are referring to in the main text.

Figure 2 – It would be nice to show some of these results as lat-lon maps instead of just zonal means, particularly for the most important quantities. Those plots would probably be a higher priority than the current Figures 3 and 4 (which could be moved to supplementary material).

Figure 3 – You could plot this Figure in percent changes instead of absolute. That might give more useful information.

Figure A2 – Are the sulfur emissions given in terms of mgS/m2/s or mg(SO2+SO4)/m2/s? Similarly for the burden (mgS/m2 or mgSO4/m2).

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