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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of harmonized emissions on aerosol properties in global models – an AeroCom experiment" *by* C. Textor et al.

Anonymous Referee #1

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1 General comments

I find the paper rather interesting, especially as it suggests that the differences in mass emission rates do not contribute too much to the aerosol model disparities. I think that the this result is quite important and I would very much like to see the article published in ACP.

Howeverm, the paper is somewhat hard to read as it seems to be very tightly connected to previous paper of the same group of authors (Textor et al 2006). This connection is close, but I think that the current paper has potential of enough individual scientific merit to be published. However, I would require the authors to carefully consider adding

(but not reprinting) in some shorter form parts of the previous paper's tables, as there is no way to read the current paper without all the time consulting the Textor 2006 paper. This should include at least the names of the models and some of their key parameters (CTM/GCM, type of aerosol module, meteorology used, etc).

I have no problems with publishing negative results of experiments (as it is now here: the harmonization of emissions did very little to reduce disparity between all models), but I would like the authors to do significant discussion on the possible causes of the remaining (still rather large) disparities.

I think that the paper fits well in ACP and addresses a relevant questions within the field of atmospheric sciences. However, the concept of the paper is not extremely novel. The paper definately suffers for looking like an appendix to the previous paper by same authors. As such, I recommend some additional analysis to be done on the basis of the data available to them. The conclusions of the paper are also suffering from the same lack of new information: they are very similar to the previous article.

For the quality of presentation, the article is very good. The language, formulae and structure of the paper are in good shape. For the figures, some of my specific comments below would require some changes into them, but in general they are well done and clear.

2 Specific comments

As it is now, the article has few key issues which I would like the authors to clarify:

1. Some of the models are clearly run with different setup from ExpA. This is well characterized in the paper. In this case leaving them out from the model diversity plots is justified. However, I would be interested to know how much of the other

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(plotted) aerosol parameters are affected by these changes?

To clarify, an example: UIOGCM used ExpA SS emissions. The authors did not plot these in SS results in figure 1, but did so for other species in UIOGCM. How much of the model diversities of other species in UIOGCM could then be due complex interactions between ExpA emissions of SS and other modelled parameters?

I understand it is impossible to reach any exact number based on this study, but it should at least be mentioned in discussion. One additional help for reader would be to use different colors for these models in figure 1. This way the reader could maybe distinquish the not-completely-ExpB models in the figures. This is especially critical as SS and DU emissions had the greatest differences between the models and their mass loadings seem to be greatest. This effect should be somewhat visible at least in UIOGCM results, as 2006 paper mentions it having more or less full aerosol dynamics.

2. Figure 1 would also otherwise be greatly improved by some knowledge of WHICH models actually have the largest differences in modeled diversities. This is connected to the later point below, but this would also be an indicator for future studies. Another way would be to print the information similarily as in 2006 paper (fig 3. in there for example).

3. Why have the authors not done all the different analyses they did in the previous paper (Textor et al, 2006) to this case with harmonized emissions? Naturally, differences in emissions should not be plotted, but I especially missed plots on fine/coarse aerosol fraction changes and zonally averaged plots of AER. These would now be much more relevant as the sources would be identical. If the plots are not significantly changed, I would at least require more detailed discussion in the text.

4. How did fine/coarse split change with the common emissions? This is indirectly

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mentioned in section 3.4, but more detailed discussion on the subject is needed.

5. (3.1 emissions)

   Page 1705, line 13: ".. or by inaccurate implementation." Is there a specific reason for this addition? Which of the models suffer for it?

   On few lines lower: ".. intermediate versions of AeroCom emission data". Again: which models and how large were the differences between these and final versions?

6. (3.1 emissions)

   Page 1706 line 1: "...differences in precursor gas emissions..". How large were these emission changes and was there any effort to harmonize these? How was the comparision of individual processes made?

7. (Spatial distributions)

   Discussion of additional layers from different height in the text would be interesting (0-1, 1-2.5, 2.5-5km). All the tables are reported with above 5 km. More details why this was done so would be appreciated.

   Same for the horizontal dispersal. The 2006 paper used polar regions for horizontal dispersal studies especially as they were least affected by emissions. So it is not very surprising that the results did not greatly change as emissions changed. The comparision is valid anyhow valid as it was done in previous paper. However, how do the horizontal dispersal change in other parts of the domains? I think that the authors should find an another way of showing the horizontal dispersal outside of the polar regions as now it should be more valid than in 2006 paper?

8. (discussion) Currently, the paper more or less just repeats the main discussion points from Textor 2006 without adding too much new viewpoints. The authors

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could give at least significant direction to future studies helping them to concentrate on possibly most important differences. The current methodology in this paper does not give easy way to see the differences in different model approaches. Note that I am not urging you to do any kind of decision which methodology is the best one for aerosol modelling, just to locate possible similarities within different approaches for the main parameters in this comparision study.

For example, I would like very much to see that the authors will group similar models together such as GCMs vs CTMs; models with resistance dry deposition vs. constant velocities; meteorology from different sources (self, nudged, ECMWF); aerosol microphysical representation : Full dynamics (ARQM, DLR, GOCART, MPIHAM, PNNL, UIOGCM, maybe ULAQ) vs. simple dynamics. If possible also Modal/bin, int/ext mixing, even they would be probably more minor effects; rough split to high resolution models (with grid sizes in order or 2 degrees or less) and others. Then the authors could see if any of the diversity plots one could find a clear groupings of these. Now, this information could be get by careful reading of the 2006 paper, this paper and comparing several plots, but the authors would make this comparision much easier for the reader.

Even if the results would not be conclusive (for example if the split in methodology would not clearly group together in any results), this should then be mentioned in text.

9. Number concentration changes. I understand that the model differences are huge in number concentrations (if it is even prognosed), but as all the indirect aerosol effects are strongly dependent on aerosol number concentrations, the knowledge of even order of magnitude of disagreement would be a great addition to this paper. Especially, as the size of the emissions is now fixed between models, this would (in theory at least) fix most of the emission disparity of aerosol number.

   Naturally such comparision should only be done in relevant models which are capable for such (models with sufficient mechanisms included).

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1699, 2007.

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